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ARTICLE I.

ON THE VIEWS OF BIOT AND WEBER

RESPECTING THE RELATIONS OF THE

HINDU AND CHINESE SYSTEMS OF ASTERISMS;

WITH AN ADDITION, ON MÜLLER'S VIEWS RESPECTING THE SAME SUBJECT.

BY WILLIAM D. WHITNEY,
PROFESSOR OF SANSKRIT IN YALE COLLEGE.

Presented to the Society October 16th, 1862.

IN the sixth volume of this Journal was published a translation, with an elaborate commentary and exposition, of the Sûrya-Siddhânta, one of the most ancient and authoritative text-books of the Hindu science of astronomy. The work soon after received a generally appreciative and gratifying notice, running through several numbers of the Journal des Savants (Paris, Aug.—Dec., 1860), at the hands of the eminent physicist and philosopher, Mons. J. B. BIOT, who, after his name had been identified for considerably more than half a century with the history of French science, has since (Feb. 3rd, 1862) died, at the great age of 87 years, active, laborious, and prolific to the very end, still claiming a place in the working ranks of the present generation, not less than in their grateful regard and admiration, as the last survivor of a band of giants in intellect whose achievements shed lustre over the first half of the nineteenth century. In the series of articles referred to, M. Biot takes up anew the discussion of the Chinese origin of the Hindu system of *nakshatras*, or lunar asterisms—a question first opened by him as long ago as 1840. This discussion it is the principal object of the present paper to continue, with reference also to the views recently set forth upon the same subject by Prof. A. WEBER, of Berlin: but, before entering upon it, I must ask permission to reply briefly to the unfavorable judgments passed by M. Biot upon certain portions of the translation and commentary of the Siddhânta, in the course of his prevailingly commendatory review. In craving this liberty I think myself justified by the consideration of the great weight of authority attaching to the publicly expressed opinions of one so universally known and honored: I am unwilling to allow our work to lie under his partial condemnation without some explanation and protest with regard to points in which I think he has misunderstood it, or judged it too harshly, and thus has done us unintentional injustice. Were he yet living, such explanation might be addressed to himself privately, leaving it to him to do us more public justice if he saw fit; but, as that is now impossible, we can only make our counter-plea before the public, and turn the case over for their final judgment.

The first matter which calls forth M. Biot's disapprobation, and upon which he lets fall a heavy burden of censure, returning to it once and again in the course of his articles, is the introductory note prefixed to our work, wherein the translator sets forth the manner in which he was led to undertake the translation and comment of the Hindu treatise, and the considerations which, in his view, rendered the execution of the task desirable, and even indispensable. He rehearses the works of those who had earlier treated of the Hindu astronomy, and points out that, notwithstanding their acknowledged value, they contained but a partial and fragmentary exhibition of the subject, while nothing had up to that time appeared which showed the Hindu science in its *ensemble*, displaying its garb as well as its substance, holding up its superstitions, its fanciful theories, its absurd hypotheses and assumptions, in the same light as its groundwork of observed fact and its mathematical form. In all this, the reviewer sees only an arrogant and reprehensible attempt to exalt the value of the work offered by depreciating its predecessors: it betrays, to his apprehension, a misunderstanding of the real value which a translation of the treatise could now have—a value purely philological and historical, and not at all bearing upon instruction and positive science. Now nothing could have been farther from the minds of the translator and those associated with him than this overestimate of their own labors and underestimate of those of others, which M. Biot reprehends so severely, and if the preface appears to breathe such a feeling, they must regret that it should be so unhappily expressed as seriously to misrepresent them. But they hope the generality of those who shall read the introductory note will find that M. Biot has misjudged its spirit; and they are even confident that his error will find its antidote in the translation which he himself, with entire good faith, offers of the passages to which he takes exception. It was not the duty of the translator to set forth in detail, and with lengthened eulogy, the merits of those who had gone before him, but only to present the considerations which justified him in taking up the subject anew, and in this particular way, notwithstanding all that they had done. He would not think of disputing an item of the praise which M. Biot, in his defense of previous writers, feels called upon to award to their works; he would only ask that M. Biot should allow the truth of his counter-allegation, that those who wished to understand the Hindu astronomy in its entirety—and especially in its historical and philological aspects, as distinguished from its scientific—were in pressing need of such a guide to its comprehension as a complete translation and annotation of one of its principal treatises would furnish. If the commendations which M. Biot, with the utmost kindness and liberality, afterwards bestows upon the work itself are at all merited, he who undertook it cannot fairly be accused of overweening self-estimation for claiming that there was both room and call for such a work.

Our reviewer expresses his decided preference for such an arrangement of the matter composing the volume as should give the translation of the Siddhânta text in unbroken continuity, leaving the exposition to follow after in a mass. I cannot think that this preference will be shared by many of those who shall have occasion to consult and use the book. Considering the want of continuous and orderly arrangement in the treatise—to which M. Biot himself calls attention, illustrating it at some length—and the obscure and elliptical character of the text, which is in great part quite unintelligible without the aid of a commentary, it is probable that ninety-nine persons out of a hundred would prefer to have each connected passage immediately followed by its own explanation, as is the case in our work. This is the method usually followed—and, so far as I know, with universal approval—in the publication of original Sanskrit texts with their commentaries; as for instance, in all the editions of the astronomical text-books, including that of the Sûrya-Siddhânta itself. The few persons who shall wish to entertain themselves by a continuous perusal of the pure Siddhânta text may well enough be called upon to take the slight additional trouble of sometimes turning over more than one leaf to find the next passage, for the sake of the many whose convenience will be consulted by the mixture of text and comment.

Notwithstanding M. Biot's objections, I cannot see that we took an unwarranted liberty in modifying in our translation the titles of some of the chapters. These titles are no integral parts of the treatise, and are found to vary somewhat in different manuscripts; and they are in a few cases so palpable misstatements of the contents of the chapters to which they are appended that a translator can hardly prevail upon himself to leave them unaltered. Thus, for instance, M. Biot, in his sketch of the contents of the Siddhânta, following the guidance of the manuscript titles, informs us that the fourth chapter treats of lunar eclipses, and the fifth of solar: which is entirely erroneous, since the fourth chapter has as much to do with solar as with lunar eclipses, nearly all its rules being essential to the calculation no less of the former than of the latter; while the fifth chapter deals merely with the element of parallax, as entering into the calculation of a solar eclipse. We have not failed to give the manuscript title of every chapter, in text and translation, and, if we modified it, to explain the reason of the modification; and this ought to relieve us from reproach, unless our modifications were for the worse, and not for the better.

In objecting to the Sanskrit index appended to our work, on the ground that it is imperfect, not making reference to every case of the occurrence in the treatise of each word indexed, M. Biot palpably confounds the duty of a translator with that of an editor. We did not publish any text of the Siddhânta, and no obligation could rest upon us to furnish an index to the text; our Index was rather an index to the notes; although in these we had, in fact, been careful to mention, and to explain, so far as we were able, every technical term which the treatise contained, besides many others, found in the native commentary upon it, or in other kindred works. To refer under each word even to all the verses of the translation where it happened to be cited in parenthesis, alongside of the English word or phrase chosen to represent it, would have been of no avail, since it was likely to have occurred in the text in twice as many other passages, in the translation of which it did not appear. M. Biot complains that under *liptâ*, for example, he is referred to chapter i., verse 28, note, where the word is not used in the text, while he finds various verses to contain it to which no reference is made. But the note referred to does contain the statement that *liptâ* is the precise synonym of *kalâ*, 'a minute of arc,' and that, while the two are employed interchangeably in the text, the former occurs much more frequently than the latter. If the authority of the translators cannot be accepted upon a point like this, if they must give a complete set of references to the original text in order to enable the mathematical reader to judge whether, after all, *liptâ* and *kalâ* do not mean two different things, then their work is not fit to be studied, and had better be laid aside altogether. They have furnished an index by the aid of which one who has a certain degree of confidence in their ability to execute properly the task they undertook may make use of their translation and notes: any other belongs to him to provide who shall study the original text, and by it shall set himself to test and correct their work. Nor does M. Biot's distrust stop short of this last step; he essays in a single point, by way of example, to convict their translation of inaccuracy, and to correct it. We are mistaken, he says, in rendering the word *bha* sometimes by 'asterism,' as if it were the synonym of *nakshatra*, since it really means only 'a fixed star in general.' Now if we had presumed to criticize one of M. Biot's formulas, pronouncing it mathematically unsound, he would doubtless have thought that we were overstepping our proper limits, and, by dealing with matters which he understood better than we, exposing our criticisms to discomfiture and ridicule. But he, in his turn, when laying to our charge a gross mistranslation, himself knowing not a word of Sanskrit, should have been very careful to see that his accusation was justly founded. In point of fact, it is entirely baseless: for *bha*, which originally, like *nakshatra*, meant simply 'star, shining heavenly body,' is in the Siddhânta employed both in this its general etymological sense, and with the specially restricted meaning of '*nakshatra*, lunar asterism.' It even much more often receives this latter meaning than *nakshatra* itself (which is comparatively a rare word, occurring but six times in the Siddhânta); a conspicuous and unequivocal instance might have been found by M. Biot at viii. 1, in the very introduction to the chapter on the *nakshatras*. Moreover, it is not infrequently applied to designate the signs of the zodiac, or the arcs of thirty degrees into which the ecliptic is divided; and only the connection, or the requirements of the case, can determine which of its three different senses it bears, and which must be substituted for it in making the translation of any given passage.

It is, of course, a legitimate matter for difference of opinion how far, in translating a work of science from a language with which scientific men are entirely unfamiliar, its technical terms should be translated. Respecting such of them as have technical correspondents in the language of the version, there would be, indeed, little or no question: others would be more doubtful. But we had so strong a sense of the inconvenience and perplexity arising from the frequent introduction, into a text intended for other than philological readers, of terms which are without known meaning, and, even if laboriously learned and made somewhat familiar, yet possess no power to suggest to the mind their significance, and require always an effort of the memory to recall the thing they designate, that we laid down for our guidance the principle that every term in the Siddhânta for which a tolerably accurate and not too tedious English equivalent could be found, should be uniformly rendered by that equivalent. At the same time, for the benefit of those scholars who were familiar with and preferred the Sanskrit terms, we scattered them with great liberality through our version, putting them in parenthesis after the words chosen to represent them. M. Biot's objections to one or two special cases of the application of this method have not convinced me that the method was not, on the whole, the most eligible one, and worthy to be consistently adhered to. Two difficulties are thus avoided. In the first place, the Hindu technical language offers a number of synonyms for almost every scientific term, and among these it would have been necessary to make a somewhat arbitrary selection. If M. Biot has objected to our rendering both *bha* and *nakshatra* by 'asterism,' which is as nearly as possible the primitive meaning of both, in virtue of which they are capable of being used to designate the same object, what would he have said to our rendering *bha* directly by '*nakshatra*'? In the second place, the misinterpretation and misuse of a term may sometimes be checked by a translation, rather than a bodily transfer, of it. It is easier to preserve from distortion in the mind a thing represented by a word which is directly intelligible, than one for which a dead algebraic sign is used. An illustration is furnished by this very term *nakshatra*, in connection with which M. Biot especially criticizes our method. If he had been constantly mindful that the proper meaning of the word was 'asterism,' and that whatever other significance it had came through that meaning, he might perhaps have been in part saved the misapprehensions of the Hindu system of *nakshatras* or asterisms which, as the case stands, he has not been able to avoid.

I would farther briefly point out—although the matter is perhaps of too small consequence to merit notice—that M. Biot's argument in favor of the possibility that the Hindus learned their astronomy from the Greeks later than the time of Ptolemy, and were not ignorant of his labors and their results, is called out only by a mistranslation of the language which we had used in reference to the subject. We had said that "the absence from the Hindu system of the improvements introduced by Ptolemy into that of the Greeks tends strongly to prove that the transmission of the principal groundwork of the former took place before his time;" and M. Biot renders our phrase "tends strongly to prove" by 'seems manifestly to prove' (*semble prouver manifestement*), and 'offers the certain proof' (*offre la preuve certaine*). One more familiar with the English idiom would have seen that the language used did not assert a certain proof, but distinctly implied the contrary: it was equivalent to saying that this was a marked indication, or a *primâ facie* argument, in favor of the fact as stated, while nevertheless it could not be regarded as establishing the truth of the latter; its force being weakened by various opposing considerations, such as those which M. Biot adduces against it, and whose bearing and pertinency we had ourselves not failed to perceive and take into account.

But that part of M. Biot's review of our work which has caused me the most surprise and disappointment, and against which I can least refrain from raising a protest, as altogether unjust and injurious, is the following paragraph, which I translate entire:

"I should be glad if I could close here the scientific examination of their work. But, to my great regret, I find myself completely at issue with them upon a point more controverted, and involved in greater obscurity, than almost any other; namely, this: wherein the twenty-eight *nakshatras* described in chapter viii. of the Sûrya-Siddhânta precisely consist, what astronomical use they are capable of filling, and whether they are originally native in India or introduced from abroad. I was led, twenty years since, to recognize, and to demonstrate by palpable proofs, that this singular institution, which enters into the general system of the Indian astronomy as a thing foreign to it, has its root and its explanation in the practical methods of the ancient Chinese astronomy, whence the Hindus derived it, altering its character, in order to employ it in astrological speculations. All the investigations which I have since been able to make into the subject have contributed to render this conclusion more manifestly true in my eyes: and, so late as last year, M. Stanislas Julien brought to light a very ancient Sanskrit-Chinese document which furnishes the most striking confirmation of it, containing a bilingual table, in which the twenty-eight Chinese *sieu* and the twenty-eight Hindu *nakshatras* are consecutively enumerated, by their own names, and set over against one another, precisely in the order of correspondence which I had attributed to them. Thus the results at which I had arrived in 1840 turn out to have been admitted and recognized, centuries ago, in China, as a matter of general opinion. But nothing of all this has touched those among the Indianists of our time who had formed for themselves in advance general theories respecting the nature and origin of the Hindu *nakshatras*. Learned as scholars, and subtle as philologists, they have yet been led astray by their want of positive knowledge. Being strangers to the methods of astronomical observation, not knowing how to discern for themselves what was physically possible or impossible to ancient observers, they have gratuitously attributed to the latter ideas which they have not had, and which even could not possibly come into their minds, since, for them, their utility would have been absolutely null, and their realization impracticable. They have not seen that the formation and astronomical application, at a very remote period, of the twenty-eight *nakshatras* of unequal amplitude which the Sûrya-Siddhânta describes, imply a mode of observation by differences of right ascension, founded upon the mechanical measurement of intervals of time, whereof no trace is found among those nations of antiquity which had been the exclusive object of their studies. Then, when they have been informed that these conditions of origin were found actually realized, not far from India, in the ancient Chinese astronomy, of which they knew nothing, they have rejected this suggestion as a kind of insult to the occult science which they had dreamed out. I shall not reproduce here the demonstrative facts upon which I have established it. It would be useless to present them anew in the same form. But, addressing myself once more to the Indianists who have rejected them without discussion, on account of their incompatibility with their own systems, I shall attempt to show them clearly wherein consist the illusions which they have formed for themselves upon the subject of our debate, illusions which could only lead them to embrace phantoms. This will be the object of a special article, after which I hope never again to have to return to this subject."

Here are several very serious and damaging charges made against the whole body of Indianists; ourselves being included, and even, as any one would naturally suppose, reckoned as the most heinous offenders of all, since it is in the course of a review of our work, and in the sequel of an asserted utter dissent from our opinion upon the point in controversy, that the indictment is brought forward. The Indianists have formed fanciful *à priori* theories respecting the origin and character of the Hindu system of asterisms. They have not known enough of astronomy to appreciate the force of the scientific arguments by which M. Biot has shown the untenability of those theories. Moreover, they have been too obstinately attached to their own previous notions, and too jealous of the honor of India in the matter, to be willing even to examine M. Biot's proofs: they have simply rejected them, and determined to continue to believe as they had believed, in spite of him and of astronomy. If they are guilty of this mingled ignorance and wrong-headedness, they doubtless deserve to be visited with the reprehension of all scholars and scientific men—which M. Biot, certainly, has done his best to call down upon their heads. That they may not, however, in too just requital of their own unworthy conduct, be finally condemned without a hearing, I shall beg leave to offer on their behalf a defence, to which a review of the history of the controversy respecting the Hindu *nakshatras* will form an appropriate introduction.

The peculiar views of M. Biot respecting the Chinese origin of the Hindu system of *nakshatras*—that is to say, of asterisms, whether single stars or constellated groups, marking out a division of the ecliptic into twenty-seven or twenty-eight parts, commonly supposed to have been suggested by the moon's sidereal revolution in from twenty-seven to twenty-eight days, and to have been established mainly for the purpose of marking approximately her daily movement—were first brought forward by him in the course of a historical sketch of the Chinese astronomy, called forth by Ideler's work on Chinese chronology, and published in 1840 in the Journal des Savants, in the form of a series of articles.* In this sketch he claimed to prove that the system in question was purely of Chinese growth, having been first established about 2350 B. C., and completed and perfected more than twelve centuries later, or not far from 1100 B. C.; that it had originally no relation whatever to the moon or the moon's revolution, being, rather, a series of single stars lying near the equator, and intended to be made use of as points of reference in observing the times and intervals of meridian transit of the various heavenly bodies, whether sun, moon, planets, or fixed stars; and that the Hindus, and other eastern nations, had imported the system from China, and had distorted it that it might be applied to uses which it was neither fitted nor intended to serve, seizing upon the chance coincidence of its number of divisions with the days of the moon's sidereal revolution to bring it into special relations with that planet. The grounds upon which this view is based need not be set forth here; they will in part come up later for statement and criticism: at present I pass on to notice the reception it met at the hands of the Indianists. By Lassen, in his Indische Alterthumskunde,* it was implicitly accepted, and made to contribute its part toward his determination of the chronological periods of Indian history. This prompt and trustful assent, however, of the chief authority of that period on Hindu archæology, entirely escaped the notice of M. Biot, who has only quite lately, in his very last discussion of the subject (1861), excepted Lassen on account of it from the condemnation he had pronounced upon the whole body of students of India. That the matter did not at this time receive wider notice and discussion is owing, as I conceive, not to the indifference and incredulity on the part of the Indianists to which M. Biot is inclined to attribute it, but to the fact that their attention was engaged by things of more pressing importance, and that the ground was not yet cleared and the way prepared for any thorough and penetrating investigation respecting it within the field of the Hindu literature. Prof. Weber of Berlin, however, in his Lectures on the History of Indian Literature (Berlin, 1852; p. 221), referred to M. Biot's researches, and took occasion to express his want of faith in their chief result, the Chinese origin of the Hindu system of asterisms, and his suspicion that the Chinese might rather have derived their own system from India; inclining, however, toward the conclusion that this mode of division of the heavens was first practised in Chaldea, and thence spread in both directions, to India and to China. In a later lecture, on the Intercourse between India and Western Countries,† the same scholar repeated in a still more peremptory manner his rejection of M. Biot's views, pronouncing them "unceremoniously to be rejected as impossible;" not founding his disbelief upon any observed and stated unsoundness in the arguments by which the French savant had defended his position, but upon certain general considerations, and upon the occurrence once or twice in the Hebrew Scriptures of a word which seemed to him to render it probable that the system was known to the Jews in the times of the Kings. This summary rejection, on the part of so eminent an authority, of conclusions so elaborately established, and so confidently entertained, and even warmly espoused, by M. Biot, appears to have touched the latter very keenly, and to have impressed upon his mind, so strongly that he was never afterward able to get rid of it, the conviction that he had no justice to expect from the Indianists. To him the grounds of Weber's incredulity seemed altogether trivial, and unworthy of being opposed to his own arguments, drawn from long-continued and painful researches in the scientific and literary history of China: his opinions, he thought, had been dismissed with a mere shrug of the shoulders, whiffed away as if unworthy of serious consideration and refutation. While it is impossible not to respect this injured feeling of the veteran astronomer and archæologist, it is also not difficult to find excuses for the apparent brusqueness and want of consideration of the German scholar. His lack of faith in the other's results was, as his later papers show, founded upon a much more careful and thorough examination of them than his language indicated; but the necessarily compressed style of his essay rendered impossible any full statement of his reasons, and he only presented such a view as would be best appreciable by those for whom he was writing. His distrust of the critical treatment of the Chinese authorities relied upon by M. Biot was strong; the frightful failures of Bailly and Bentley to found a history of Hindu astronomy on a basis of mathematical calculation had rendered justly suspicious in his eyes that whole mode of investigation; and, having made up his adverse judgment, he expressed it with the freedom and directness which are his wont, and which, though sometimes, perhaps, wearing the aspect of dogmatism and over-confidence, really cover as much pure love of truth, freedom from prejudice, accessibility to the opinions and arguments of others, and candid openness to conviction, as are to be found in any scholar of the present day. To Prof. Weber's condemnation of his views, M. Biot put forth a reply in the Journal des Savants for 1859: he there furnished, in the form of a series of articles* on the Oriental Astronomer of our late learned associate, Rev. H. R. Hoisington, as full an exposition of the Hindu astronomical system as could be drawn up from the materials accessible to him. The last article of the series is devoted to the subject of the *nakshatras;* the author restates in full, although concisely, the results at which he had arrived twenty years earlier, and the considerations upon which they had been founded, and endeavors, at the same time, to prove the objections alleged by Prof. Weber of no value or cogency.

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* The articles are six in number; the first is given in the Number of the Journal des Savants for December, 1839, and the last in that for May, 1840. The title of the separate edition is Recherches sur l'Ancienne Astronomie Chinoise, publiées à l'occasion d'un Mémoire de M. Ludwig Ideler sur la Chronologie des Chinois: it occupies ninety-eight quarto pages.

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* Vol. I, Part 2 (1847), p. 742 etc.

† Die Verbindungen Indiens mit den Ländern im Westen; published in 1853 in the Kieler Monatsschrift, and later in Weber's Indische Skizzen, Berlin, 1857.

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* These articles, six in number, are to be found in the Journal des Savants for April to September, 1859; in the separate impression, dated October 1859, they fill

It was at this time, in the summer of 1859, that my own investigations into the Hindu system were commenced, in connection with the Sûrya-Siddhânta. My attention was at once, of course, attracted to the views of M. Biot upon the subject, and they engaged my warm interest and my careful study. The form in which they were presented, as was the case with everything proceeding from their author's clear mind and eloquent pen, was very engaging, and the scientific basis upon which they claimed chiefly to rest had the appearance of being well and securely laid, while the adverse arguments by which they had been assailed seemed to me meagre, and of doubtful force. It was, indeed, *à priori*, a strange and hardly credible thing that India should have borrowed so important a portion of its ancient science from the far-off and almost inaccessible China, yet it was evidently not utterly impossible, and would admit of being proved by sound and sufficient evidence. If M. Biot, who had enjoyed greater advantages for studying the Chinese astronomy, and had devoted to it more attention, than any person living, had read its history correctly, the question of the origin of the system was fully solved, and it only remained for us to frame our views of the relations subsisting among ancient nations, and of the communication of knowledge from one to another, in a manner to square with this most important fact. My opinion to this effect was expressed before the Society at its meeting in New York, three years ago, and the substance of it

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ninety-six pages. They are also reprinted, along with two other later series of articles by the same author—that on the Sûrya-Siddhânta and that on the History of Chinese Astronomy, to be noticed later—in a separate volume, entitled Etudes sur l'Astronomie Indienne et sur l'Astronomie Chinoise, par J. B. Biot etc. (Paris, 1862. 8vo). This volume, which was not issued from the press until after the author's death, has, with great impropriety, been extensively advertised as a posthumous work; while in fact, with the exception of an interesting but unfinished introduction, upon the scientific value of the Egyptian astronomy, it is entirely made up of *verbatim* reprints from the Journal des Savants—a fact which the editor, on his title page, in his introductory note, and everywhere else, has suppressed, or, at least, carefully ignored.

An article of sixteen pages, on Albiruni's account of the *nakshatras*, published in the Journal des Savants for January 1845, should also be mentioned here. It is not included in the reprint.

printed in the account of the proceedings at that meeting.* Maturer consideration of M. Biot's views and arguments, however, and especially a deeper study and better comprehension of the internal relations of the Hindu system itself, somewhat shook my confidence. I saw clearly that one whole department of the evidence on which he rested his case would have to be ruled out as irrelevant. He had alleged that the Hindu system was in part proved to be of Chinese origin by the fact that the Chinese employed it for what was palpably its true and original purpose, while the Hindus misapplied it to uses for which it was as ill fitted as an auger for cutting a stick of wood in two, or a saw for boring a hole. This argument, it was plain to me, rested upon a fundamental misconception of the character of the Hindu asterisms, to which M. Biot had unwittingly transferred a part of the attributes of the Chinese *sieu:* the former were, in fact, at the least as well adapted to the uses made of them as were the latter. If M. Biot's theory, then, was to be accepted, it was solely upon the ground of his having proved the *sieu*, upon sufficient historical evidence, to be an institution of Chinese growth and development. To enter upon Chinese ground, and to reopen the investigation there, was what I had neither time, inclination, nor ability to do. In the notes, accordingly, to the eighth chapter of the Sûrya-Siddhânta, we set forth, as fully and plainly as was in our power, M. Biot's whole argument and conclusions, and gave in a qualified adhesion to the latter, putting our assent distinctly upon the ground of his researches into the history of the Chinese science, and pointing out wherein he had done injustice to the Hindus, and wherein his apprehension of their system needed correction: pointing out, further, considerations which appeared to us to indicate, almost too strongly for question, that the supposed communication between India and China could not have been direct, but must have taken place through the medium of some third people. Our general conclusion was expressed in the following words:

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* As these Proceedings, although extensively distributed at the time among the members and correspondents of the Society, were never published in its Journal, I cite here the paragraphs relating to the subject:

" Prof. Whitney then farther adverted to the theory of M. Biot—developed in the Journal des Savants for 1849, and re-stated in that for August of this year—that the ancient Hindu division of the ecliptic into 27 or 28 *nakshatras* or lunar mansions, which is much older than the proper Hindu science of astronomy, came to India from China, and read, from a letter to himself of Prof. Weber of Berlin, the latter's objections to that theory, as follows:

"'In still adhering to his theory of the Chinese origin of the Hindu *nakshatras*, M. Biot fails to note 1. The radical connection of the Hebrew *mazzaloth* and the Arabic *manzil*, which renders it highly probable that both denote the same thing; 2. The fact that the Hindu *nakshatras* are originally 27, and not 28; 3. The great indeterminateness which characterizes the most ancient lists of their names—so, especially, in the Kâṭhaka, the Tâittirîya-Brâhmaṇa, etc., the names are altogether diverse; 4. The great uncertainty which for that reason exists also with reference to the identity of the corresponding star-groups; 5. The purely native names of the 27 *nakshatras* in the Bundehesh, which, though of Semitic origin (for whence should come Chinese influence here?), yet have their own Persian appellations; 6. The influence of the Buddhists, which has transplanted to China so much that is Indian (as is farther instanced by Julien's latest discovery of the Indian fables in Chinese translations), while no transfer in the other direction is discoverable.

"'I must confess that I am somewhat inclined to general scepticism as regards Chinese statements. I do not believe that we are to accept as genuine everything which the Chinese savants lay before us. With reference to the present they lie, so to speak, "like a book:" how much more, then, when the question is of hundreds, and even of thousands, of years ago. The epoch of historical criticism will yet dawn, for this department of antiquity also. As to the *nakshatras* themselves, I acknowledge that for me the subject is still involved in much obscurity. . . .'

" While disclaiming to enter deeply at present into the discussion of this subject, or to speak with confidence respecting it, Prof. Whitney remarked that the considerations offered by Prof. Weber were only presumptive, and some of them of doubtful value: that they were the archæological and philological arguments against the antecedent probability of M. Biot's theory, while they did not at all invalidate the force of the evidence presented in its favor, which seemed almost or quite conclusive, if unassailable by scientific argument. He also said he believed that thus far the general truth of the Chinese chronology and history had only been confirmed by the researches of modern critics."

> "We would suggest, then, as the theory best supported by all the facts thus far elicited, that a knowledge of the Chinese astronomy, and with it the Chinese system of division of the heavens into twenty-eight mansions, was carried into Western Asia at a period not much later than 1100 B. C., and was there adopted by some western people, either Semitic or Iranian. That in their hands it received a new form, such as adapted it to a ruder and less scientific method of observation, the limiting stars of the mansions being converted into zodiacal groups or constellations, and in some instances altered in position, so as to be brought nearer to the general planetary path of the ecliptic. That in this changed form, having become a means of roughly determining and describing the places and movements of the planets, it passed into the keeping of the Hindus—very probably along with the first knowledge of the planets themselves—and entered upon an independent career of history in India."

Later in our work, in order to do the utmost possible justice to M. Biot, by completely illustrating his views and the grounds on which they were based, I caused to be engraved and printed a comparative chart of the three systems, Chinese, Arabic, and Hindu, which I had prepared, in great measure for the purpose of drawing upon it the equator of B. C. 2347, regarded by him as having played an important part in directing the first selection of the Chinese determining stars.

That, after this elaborate statement, discussion, and illustration of his views on our part, and notwithstanding our partial assent to them, M. Biot could accuse us of having formed in advance a theory respecting the nature and origin of the Hindu *nakshatras* discordant with his own, and of having sacrificed to it all his laborious researches and their results, without even taking the trouble to examine and endeavor to understand them, is a thing entirely incomprehensible to me, or comprehensible only on the supposition that his mind was so poisoned by the previous curt denial of his theory on the part of a single scholar as to be unable to distinguish between a partial acceptance and a total rejection, and that he was so persuaded of the infallibility of all his own arguments that he could conceive only of ignorance and carelessness as dissenting from them. But to require that we should put our minds under his despotic control, and dare to hold no opinions, even respecting matters of which we ought to be better qualified to judge than he, except such as he prescribed to us, even M. Biot, were he twice as eminent an archæologist and astronomer as he was, had no right. That he should be convinced by our arguments upon points respecting which we differed from him, and should modify his opinions accordingly, was what we, on our part, might wish and hope, but could hardly venture to anticipate, and certainly did not presume to demand; but we were, it seems to me, fully justified in expecting that he would not absolutely ignore our stated grounds of dissent, and even explicitly deny their existence, by accusing us of blindly and obstinately adhering to our preconceived views, in defiant neglect of the better light which he had tried to afford us. In fact, nothing can be plainer than that we, and not M. Biot, have a right to complain of a want of attention to the results of our labors. In a final article on the *nakshatras*, following next after the paragraph of which the translation was given above, he proceeds to set forth for the third time his views respecting their character and origin, in almost precisely the same manner, and upon precisely the same ground-work of evidence, as he had already done twice before. Throughout the whole discussion, although introduced into a professed notice of our work, he makes not the least account of or reference to the latter, nor lets slip a tittle of evidence that he had ever examined it. Our objections to certain specific points in his reasonings get no answer, and the points are reiterated and urged anew as conclusive, as if their force had never been impugned. Colebrooke is loaded with commendation for having made honest use of the means at his command, and identified as well as he could the groups composing the *nakshatras*, not yielding to any temptation to misrepresent them, from a foreboding that his results might some time be used as evidences of the Chinese origin of the whole system: our own corrections of Colebrooke's identifications, which are in one or two points not without an important bearing, now favorable and now unfavorable, upon M. Biot's conclusions, are passed without notice. And M. Biot proceeds to draw out an exposition of the character and history of the

Hindu asterisms which is entirely at variance with our understanding of them, derived from the Sûrya-Siddhânta and other native documents, and set forth in the commentary on the Siddhânta; and he supports it with considerations which are in several instances directly opposed to the teachings of the Siddhânta itself, as interpreted by us. To this exposition I shall presently return: I pass on now to a brief account of the later contributions to the discussion of the same general subject.

Before the completion of M. Biot's series of articles on the Sûrya-Siddhânta, Prof. Weber had published the first part of a memoir in which he undertook to explain and defend the ground which he had taken earlier in opposition to the views of the distinguished French savant.* In this he attacked the very groundwork of evidence on which his adversary's history of the Chinese astronomy had been founded, attempting to show that the Chinese system of *sieu* was not traceable farther back than to two or three centuries before Christ, a period when, in his view, Hindu influence upon China would admit of being presumed; and arguing that, whatever might have been the origin of the *nakshatras* themselves, the *sieu* were derived directly from them. In answer to the open assault of Weber, and also to certain misgivings expressed in our notes to the Sûrya-Siddhânta respecting the documentary evidence sustaining Biot's history of the Chinese astronomy, the latter replied with a new exposition of that history; published, like his other works upon the same subject, in the Journal des Savants, as a series of articles.† Prof. Weber, finally, has closed up the controversy, since the lamented death of his opponent, by the publication of the second part of his memoir, in which he presents the results of an extended and thorough investigation of the position and value of the *nakshatras* in the whole ancient religious literature of the Hindus, laying for the first time a solid foundation for our knowledge of the value of this element in their science and their superstition.‡ As, however, I can no more agree with his opinions respecting the relation of the Hindu to the Chinese asterisms than with the opposing ones of M. Biot, and as I cannot admit the relevancy and force of all the arguments used by either party, I shall go on to criticize the whole discussion, and to set forth my own persuasions and conjectures respecting the difficult and interesting point in ancient history to which it relates.

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* This memoir is to be found in the Transactions of the Berlin Academy for 1860, pp. 283-332. A separate edition of it is also published. Its title is Die Vedischen Nachrichten von den Naxatra (Mondstationen). Von A. Weber. Erster Theil. Historische Einleitung.

† They are six in number, and run through the parts of the Journal des Savants for May to October, 1861. The separate edition occupies ninety pages, and has for its title Précis de l'Histoire de l'Astronomie Chinoise. The articles are also reprinted in the volume on the Hindu and Chinese astronomy to which reference has been made in a preceding note (p. 10).

‡ This second part of Weber's memoir occupies pages 267-400 of the Transactions of the Berlin Academy for 1861, and is also to be had separately.

I commence with M. Biot. In his last, as in his former expositions of his views, this savant rests his belief of the Chinese origin of the Hindu *nakshatras* upon two grounds: first, the palpable and utter unsuitedness of the system to the use to which the Hindus applied it, and its as evident adaptedness to its Chinese employment; and second, the direct proof, documentary and scientific, that it is native in China; its appearance in India being of such a character and of such a date as readily to admit the hypothesis of its importation into the country from abroad. The former of these two foundations of his argument he sets forth in the concluding article of his series on the Sûrya-Siddhânta, and to it we will first direct our attention.

As has already been remarked, the pertinency and validity of this whole side of the argument of M. Biot was explicitly denied by us in the notes to the eighth chapter of the Siddhânta, and the attempt was there made to point out the misapprehensions from which he had derived it. Its repetition with the same confidence as before, in disregard of our objections, and without any endeavor to remove their force, renders necessary a more detailed discussion of the point than was formerly given.

It may have been, after all, not without some reference, although unacknowledged, to our counter-reasonings, that M. Biot has finally shifted his ground a little, and, no longer denying that there is a certain use of the *nakshatras* in which their application is not so entirely absurd, marks this as a modern modification, distinguishing it from the ancient form of the system, which remains open to all the imputations which he had formerly urged against it. He heads his article "On the Ancient and Modern Nakshatras of the Hindus," and commences it with the following statement:

> "Since the first condition to be complied with in order to the proper treatment of a philosophical subject is its distinct limitation (*la circonscrire nettement*), I begin with declaring that I here employ the terms 'ancient' and 'modern' in a sense purely relative. I call 'ancient *nakshatras*' the twenty-eight of unequal amplitude which are described in the eighth chapter of the Sûrya-Siddhânta, and in the other classical treatises of Hindu astronomy derived from the same type, as being in use at their period, without any mention made of earlier *nakshatras*, which may have been differently constituted. I call 'modern,' on the other hand, the twenty-seven of equal amplitude which have been more recently substituted for the others, and which have since been, and are still at the present day, the only ones practically employed in India."

This distinction of "ancient" and "modern" *nakshatras* I hold to be entirely fictitious, having no foundation whatever in the facts of the Hindu science, but only in M. Biot's misapprehension of those facts. He has taken the teachings of the astronomical text books, distorted a part of them, imagined others to complement them, and then made a division of them into two parts, setting down some as belonging to an ancient system, and others as belonging to a modern system, upon no other ground, that I can discover, than his own arbitrary choice; thus converting into two discordant institutions, of different date, what is in reality only one and the same thing. That he was not perfectly honest in all this, and did not put forth what he believed to be the true account of the *nakshatra* system, no one can for a moment suspect: but it is certain that his preoccupation of mind in favor of his own peculiar theory must have been very absorbing, or he could never have framed for its support so extraordinary a misrepresentation. Let us see for ourselves what aspect the asterisms wear in the Sûrya-Siddhânta, the native authority upon which M. Biot was solely dependent—he assuming, as he had sufficient reason to do, that its teachings agreed with those of the other treatises of the same class.

It should be remarked by way of preliminary that the Siddhânta gives no complete and connected exposition of the system of asterisms, stating the number, names, and order of its members, the number of stars composing them, and the like: they are assumed to be so familiarly known as to need no such attention. The usage of the treatise is the same with regard to a variety of matters of a kindred character, as the signs of the zodiac, the years of Jupiter's cycle, the months, the days of the week, etc., etc.: rules are laid down implying a vast deal of knowledge concerning all these which the Siddhânta itself does not take the trouble to give. The information respecting the asterisms which we do not find here must be supplied from sundry other sources, and its correspondence with the implications of the Siddhânta inferred from the occasional references which the latter makes.

The first passage, then, where anything is taught in the Sûrya-Siddhânta respecting the *nakshatras* is near the end of the second chapter, after the completion of the rules for calculating the true places of the planets. It is there (ii. 64) simply said that the portion (*bhoga*) of an asterism—i. e., the part of the ecliptic or planetary path belonging to each asterism—is eight hundred minutes of arc; and that, in order to find in what asterism any given planet is, the longitude of the latter, reduced to minutes, must be divided by eight hundred; this will determine the asterism, and the point in it, occupied by the planet; and hence, by means of the rate of daily motion of the planet, as found by rules already laid down, may be learned the time it has spent, and the time it has yet to spend, in the asterism. This clearly implies a division of the ecliptic into twenty-seven equal portions (800′×27=360°), each of which gets its name from one asterism, being the portion of the planetary path belonging to the latter; we had been taught before that the series begins from the end of the division Revatî; which point, again, we learn from the eighth chapter to be situated ten minutes (10′) east of a certain star in the asterism Revatî, known by us as ζ Piscium. This is the only rule which the Siddhânta gives for ascertaining the presence of a planet in an asterism; if we desire to know when the moon is in Rohiṇî, or the sun and moon together in Âçleshâ, or Jupiter in Anurâdhâ, or anything of the kind, this is the method which we must follow. In a later part of the work, the seventh chapter, is taken up the subject of planetary conjunctions. Two planets, we see from the rules laid down, are said to be in conjunction (*yoga:* there are several other terms also of synonymous meaning) at the instant when they are upon the same secondary to the prime vertical, or upon the same great circle passing through the north and south points of the horizon. This is a peculiar mode of viewing the phenomenon of conjunction: we might rather have expected it to be regarded as taking place when the two bodies had the same longitude, or the same right ascension. Whether any other people has reckoned conjunction in a like manner, or whether any historical connection is inferrible from the peculiarity, I do not know. As data for the calculation of the conjunction are given the longitude and latitude of the two planets, and the process by which the moment of conjunction is determined is a very intricate, awkward, and inaccurate one, as is fully set forth in our notes to the chapter which teaches it. The object of the calculation appears to be purely astrological; the conjunction, as we are informed at the end of the chapter, receives its title and its significance from the degree of approach of the two heavenly bodies, from their relative position, and from their comparative brilliancy. In the eighth chapter, then, which is entitled in the MSS. "chapter of the conjunction of the asterisms and planets," the Siddhânta goes on to teach us how to determine the instant of a like momentary conjunction, on a secondary to the prime vertical, of any given planet with any one of the asterisms. The mode of making the calculation having been already sufficiently explained in the foregoing chapter, it was only necessary farther to give such a definition of the positions of the asterisms as should furnish the data requisite for performing the process. This is done in the manner which has been repeatedly described and illustrated, by M. Biot himself among the rest: the star is referred to the ecliptic by an hour-circle, and its distance from the ecliptic upon that circle, and the distance of that circle from the initial point of the sphere, are noted and defined. The same data are in a later part of the treatise, the ninth chapter, prescribed to be employed in fixing the times of heliacal rising and setting of the asterisms: other than these, no uses of them are anywhere hinted at. But the asterisms are well known to be in most cases constellations or groups, and not single stars; how then does their position admit of being defined in the manner here described? This is a difficulty which it is probable that the Siddhânta itself did not originally clear up; but in its present form, in a passage of the eighth chapter (viii. 16–19) which we have seen good reason to suspect of interpolation, it informs us to which of the stars in each group the definition of position applies, or which is the "junction-star" (*yogatârâ*, star determining the *yoga* or conjunction); it being quietly assumed, in the manner already referred to, that we are familiar with the constitution of the groups, and know that one star in each has been singled out to represent the whole asterism in the calculation of conjunctions.

This is the complete story of the dealings of the Sûrya-Siddhânta with the asterisms: we are now prepared to see how much of M. Biot's theory is actually found in the documents from which he supposes himself to derive it, and how much is of his own independent devising.

In the first place, there is nothing in the Siddhânta which teaches or implies that the one mode of treating the asterisms is more ancient, and the other more modern. Both are described together, and in no such manner as seems to contemplate a difference of date, or a discordance of any kind, between them. If the discordance which M. Biot assumes is to be established, it must be by evidence brought in from other quarters; and I hope to be able to show that there is no plausible evidence in its favor, it being derivable only from a misapprehension of the Hindu system, and an antecedent conviction of the derivation of that system from the Chinese *sieu*.

For, in the second place, the treatise gives not the slightest intimation that in the second passage, where the definitions of position of the junction-stars are laid down, any division of the ecliptic into portions is intended. Biot constantly assumes that *the circles of declination passing through the junction-stars cut up the ecliptic into those portions which constituted the ancient system of lunar mansions*, established for the purpose of marking the daily progress of the moon: and this assumption constitutes the main pillar of that part of his argument which we are now considering. But no such thing is to be found in his authorities, and, so far as I can see, he has obtained it only by a transfer to the

Hindu *nakshatras* of some of the characteristics of the Chinese *sieu*. The latter are, in fact, of this character: the divisions of the heavens are marked and limited by circles of declination passing through the successive single stars of the series: when a planet arrives at one of these circles, it enters into the *sieu*, or 'mansion,' bearing the name of the star with which it thus comes into conjunction; remaining therein until it arrives at the next circle of declination, when it quits the *sieu* in question, and enters the one succeeding, and so on. That the Hindu method of division was ever of this kind I see no reason for believing. The point, however, is one which requires to be examined at some length, since, strangely enough, nearly the same assumption is made by Weber, and constitutes a hardly less cardinal point in his argument than in that of Biot. Weber, too, regards the irregularity of intervals subsisting among the stars whose positions are defined in the astronomical text-books as conditioning an irregular division of the ecliptic into mansions: an equal division, in his view, implies a series of equidistant stars; and wherever he finds reference made to mansions of equal extent, he assumes the recognition of such a series, differing essentially, if not totally, from the groups which constitute the system as we know it. He would not, doubtless, insist upon bringing into the account so exact modes of measurement as by circles of declination, but would hold that, for instance, the moon entered the mansion Açvinî upon passing the principal star, or junction-star, of Açvinî, and continued there until she came to the principal star of Bharaṇî, and so on. What justifies this assumption, Weber, like Biot, entirely omits to inform us: a most unfortunate and even reprehensible omission, as it seems to me, considering the extreme importance of the point in question; for, if deprived of it, he loses the foundation which he needs to give stability to most of his other arguments. I am the more surprised that he has failed to fortify himself in this part of his position, because, in our notes to the Sûrya-Siddhânta, we had pointed out the assumption of this mode of division as one of M. Biot's chief errors; thus indicating at least the possibility of doubting its justness in view of all the evidence which had been up to that time elicited. Prof. Weber does, indeed, in one place (p. 316), refer to our view, that the *nakshatras*, instead of being limiting stars, are zodiacal constellations, marking out divisions of the ecliptic by their proximity to them, and he promises that the sequel of his essays shall show its falsity: but I look there in vain for any such demonstration; I only find that he everywhere tacitly accepts and argues upon the other view. We are left, then, to find out for ourselves whence it comes, in the one case as in the other. I have already expressed the conjecture that, with Biot, it was an unconscious ascription of Chinese characteristics to the Hindu system: I can only suspect farther that Weber may have accepted it from Biot without questioning or testing its authority, and may have made it so long and so undoubtingly an element in his views and reasonings that any other idea does not occur to him as possible. That neither derived it from the Sûrya-Siddhânta is very certain: that treatise does not contain it, nor any trace of it, nor any intimation that it was ever known or conceived. As has been seen above, the definition of position of the junction-stars (*yogatârâ*) of the asterisms is made in order to the calculation of the conjunction (*yoga*), and this is not treated as the commencement of the planet's continuance in the asterism; it is merely a momentary phenomenon, an aspect of the two heavenly bodies concerned which lasts for an instant, and then is past and gone. The moon, for instance, spends always about a day in the domain of each asterism, the exact time being determined by the rate of her advance in longitude: about once in each day, moreover, she comes momentarily into a state of conjunction (*yoga*) with the asterism itself, considered as a heavenly body, and represented by one of its stars, usually the most brilliant among them. Nor, as one would infer from M. Biot's assumption, does the *yoga* or conjunction occur when the planet reaches the circle of declination passing through the junction-star; were it so, the Hindu astronomers would have been saved a world of trouble, and not a little discredit, which their lumbering and inaccurate process of calculation of the actual conjunction upon a secondary to the prime vertical brings upon them. So ignorant, indeed, is the Sûrya-Siddhânta of any implication in its eighth chapter of a mode of division of the ecliptic different from that one which had been taught in its second chapter, that it even gives its definitions of position upon the basis of the equal division. We are not told, for instance, that Rohiṇî is forty-nine and a half degrees from the initial point of the sphere, but that it is fifty-seven times ten minutes (10′×57) distant from the beginning of its own portion of the ecliptic: that is to say, that it is nine and a half degrees from the end of the portion called Kṛttikâ and the beginning of the portion called Rohiṇî. And so with all the rest; and even more strikingly in the case of some of the latest members of the series, two or three of which fall into the wrong portion, while one has no portion at all. These untoward circumstances cause no difficulty to the author of the treatise; he coolly declares that "[the asterism] Uttara-Ashâḍhâ is at the middle of [the portion] Pûrva-Ashâḍhâ, and [the asterism] Abhijit at the end of [the portion] Pûrva-Ashâḍhâ," and so on, without the slightest suspicion of the frightful confusion he is making, on M. Biot's theory that he is here laying down a scheme of division of the ecliptic into arcs running from star to star. His implication clearly is that, however he may define the positions of particular stars, he cannot cause any difficulty, or expose himself to be misunderstood, since everybody knows that the ecliptic is divided into twenty-seven equal portions, named in succession from the twenty-seven asterisms, and the idea of a mode of division discordant with this would not suggest itself to any one's mind.

This is the aspect of the case presented in the Sûrya-Siddhânta, and, presumably, in the other treatises representing the modern astronomical science: if any of these shows the subject of the conjunction and the definition of position of the junction-stars in a different light, it has not been pointed out; nor does either Biot or Weber make reference to such authority as sanctioning the view they take. As regards the older literature, the exceedingly laborious, careful, and far-reaching excerption of ancient authorities made by Weber in the second part of his essay enables us to say with confidence that they yield no support to the theory: at least, a deliberate and unbiassed search among them has not shown me that they yield any; I can find no expression which appears to require or to suggest a mode of division from star to star. For the most part, the language used is indefinite, and must depend for its interpretation upon evidence from without: a presence of the moon or other heavenly body in a *nakshatra* is stated, without anything to tell us how the presence is to be understood, whether as implying position between the asterism named and its next neighbor eastward, or situation in that part of the ecliptic which lies adjacent to the asterism. The latter is, to say the least, fully as admissible an interpretation as the former.

Having thus seen that the theory which we are considering finds no support from the native Hindu authorities, we should be fully justified in setting it aside as undeserving of belief, until those who entertain it shall inform us upon what other grounds they base its credibility and claim to acceptance. The burden of proof lies entirely with them. Since, however, it is impossible to summon them to declare the reasons of their faith, we may well enough go on independently to inquire whether such reasons are anywhere to be discovered; or whether there may not be found, on the other hand, weighty considerations which oppose or forbid the adoption of their view.

The most important and decisive fact, which renders it well-nigh impossible that the Hindus can ever have measured their mansions from asterism to asterism, is this: that the Hindu system is from its inception one composed chiefly of groups or constellations. Had we in India, as in China, a series of single stars, there would be some plausibility in the assumption that they divided the different mansions from one another; but the presence of groups leads us almost inevitably to the conclusion that the division intended was into portions more or less nearly occupied, covered, or pointed out, by the constellations selected; that is, a division of the same kind which we find in the astronomical text-books. To measure from one *constellation* to another, taking the arc intercepted between them, is a proceeding little less than absurd. That the members of the system were actually groups from the very commencement of their history in India need not be proved here, for it neither has been, nor is likely to be, denied by any one: the earliest names and descriptions indicate it too clearly to leave any doubt upon the point. Nor is Weber able to point out a single reference to a *yoga*-star, or an intimation of the selection of such a representative of the asterism, in any authority older than the modern astronomical text-books, whose apprehension of the purpose for which the selection was made is what we have seen it to be above.

Another circumstance telling with some force against this theory is the liberty taken by certain of the astronomical authorities of making, under the same series of stellar groups, special irregular divisions of the zodiac. I refer to those which Biot first cited from Brahmagupta and Varâha-mihira (in the Journal des Savants for 1845, pp. 49 etc.), and to which he makes repeated reference in his later articles; Weber treats them in his first essay (p. 309 etc.), and finds traces of them at a date earlier than the scientific reconstruction of the Hindu astronomy. The real ground and meaning of these strange divisions is as obscure to me now as at the time when our Sûrya-Siddhânta was published; but I derive from them the same conclusion as then: that these authorities—who all held, so far as we can judge, the same actual stellar groups with which we are familiar to constitute the series of asterisms—knew nothing of any restriction of the divisions of the ecliptic to the arcs intercepted between star and star. Weber is himself struck by the fact, for which he can furnish no explanation, that the same works of Brahmagupta and Varâha-mihira which give those peculiar divisions of the ecliptic, lay down the positions of the asterisms accordantly with the Sûrya-Siddhânta, in palpable ignorance that they are committing any inconsistency or discordance. A strange obliviousness this would be, indeed, if they really meant to divide the ecliptic from star to star; and yet not stranger than that of the Sûrya-Siddhânta, which teaches the same irregularity of intervals between the asterisms along with an equable partition of the planetary path; but all difficulty disappears, in each of the three cases alike, as soon as this arbitrary theory respecting the mode of division is relinquished.

Again, we may refer to the starting-point assigned to the modern systems as a noteworthy fact bearing a like significance.

We can speak only of the modern form of the series, since the more ancient one, commencing with Kṛttikâ (the Pleiades), has nowhere had its initial point so fixed by a definition of position that we can tell precisely where it was. In later times, the first asterism is Açvinî (the head of Aries). The commencement of that part of the ecliptic constituting its portion is a fixed point, and the point of highest importance in the heavens, since, in the belief of the Hindu astronomers, the movements of all the planets at the creation began precisely there, and since they all return to a conjunction there from interval to interval, as long as time shall last. If, now, the recognized and prevailing mode of partition of the ecliptic had been from star to star, we should expect to find at the chosen initial point the principal star of the asterism Açvinî, the one which gave name to the first division. Even if the old method was to be given up in favor of a system of equal arcs, we should look to see at least so much respect shown it as would cause the first point in the new division to coincide with a point of the same name in the ancient division. But, instead of finding at the initial point of the sphere the junction-star of Açvinî, we find that of Revatî, so that the whole division Revatî lies west instead of east of its determinant: And the Sûrya-Siddhânta and Çâkalya-Sanhitâ so ignore the desirableness of commencing an asterismal portion from a junction-star, that they make the initial point of the sphere to lie, not precisely at ζ Piscium, but ten minutes (10′) east from it.

Once more, we may adduce the analogy of the Arab system as an argument against the theory under discussion. Between the *manâzil* and the *nakshatras* exists a much closer kindred than between either of them and the *sieu*. Both the two former are chiefly groups of stars, and both are brought into express connection with the moon's revolution—features which either never characterized the Chinese system, or have long since been entirely obliterated from it. But we have no account of an Arab division of the ecliptic by circles of declination, or any other means, connected with single stars; the equal portions of the planetary path are marked by the adjacency of the constellations from which their names are derived.

There remains, so far as I can see, but one ground on which the view of Biot and Weber could possibly be defended. If the mode of division from star to star is obviously the more natural, if it is the one which seems pressed upon us by general considerations of fitness, and which we should, *à priori*, confidently expect to see adopted, then, in spite of all the arguments adduced against it, we should still need to inquire whether it might not have been followed at the outset, and later replaced by the other method. So far, however, from believing this to be the case, I should maintain precisely the contrary. The analogy of the *sieu*, indeed, appears to lend it a certain degree of support: but then, on the one hand, the *sieu* are single stars, and thus fitted to stand as the limits of division; and, on the other hand, they were employed as assistants in regular and skilled observation, aided by the use of instruments. We have no reason whatever to believe that the Hindus who first employed the *nakshatras* possessed instruments, and had elaborated a system of observation of the heavens; their studies, beyond all question, were made with the eye alone. The closest analogue of the *nakshatras*, as already pointed out, is to be found in the Arab *manâzil;* but a marked similarity, in origin and application, is also to be recognized between them and the signs of the zodiac. These, too, are constellations, or groups of stars, marking out the ecliptic into twelve equal divisions; but the limits of the divisions are far from coinciding with the boundaries of the stellar spaces covered by the constellations. Some of the latter occupy forty to fifty degrees of the ecliptic; others no more than twenty; and the Scorpion, after the separation of the Balance from it, not above ten degrees. Some fairly lie upon and embrace the sun's path; others only touch it with their northern or southern edge; and in one instance a constellation overlaps the ecliptic without being therefore reckoned as one of the zodiacal series. Spite of these irregularities, the system perfectly answered its purpose, which was that of furnishing a sidereal basis for a twelvefold division of the ecliptic, and for so marking the successive portions of that circle that, if a planet were described as being in the Twins, the Virgin, the Scorpion, or any other sign, no one could doubt where in the heavens it was to be looked for. Precisely of such a nature I conceive the *nakshatras* to have been. Upon this point there need be no discordance of opinion even among those who continue to hold the most irreconcilable views respecting the ultimate origin of the Hindu system. Whatever the source from which they came, they appear clearly to have borne to the apprehension of the Hindus the character here pointed out; whether Hindu ingenuity devised them, or whether they were an importation from Babylon, or an altered and corrupted form of a Chinese institution, they were—their derivation being forgotten, or ignored, or, as M. Biot insists on our believing, fraudulently concealed—a series of twenty-seven or twenty-eight stellar groups, serving as basis for a division of the ecliptic into the same number of portions, as nearly equal as the unassisted eye could measure them —or, if sometimes considered unequal, made to supplement one another's inequalities—and regarded as suggested by the moon's revolution and established to mark her progress from day to day through the sky. It may, moreover, be confidently maintained—both against Biot, who thinks that the series could not possibly have assumed its present form if originally intended to mark the moon's daily motion, and against Weber, who holds that to mark out equal spaces traversed equidistant stars would necessarily have been selected—that the system of asterisms is, in the main, as well suited as any that could be devised to just the purpose of defining the daily stages of the moon's revolution, for a people whose only instrument of observation was the eye. Suppose that such a people, having noticed the moon's nearly equable movement in a certain path through the heavens, and the completion of her revolution in twenty-seven or twenty-eight days, and feeling the impulse to mark and define the stages of her progress, should set themselves to make choice of a means of definition among the stars through which she passed, what would they naturally demand in their selection? I answer: they would be likely to look for groups of stars, as conspicuous as the heavens could furnish in the proper position, so equally distributed that no considerable part of the series should vary from the average place required by a division of the path into nearly equal portions, and not too far removed in either direction from the ecliptic—and that is all. A succession of single stars would not probably have been pitched upon by them; because, in the first place, single stars of any brilliancy, in the desired positions, are absolutely not to be found in the sky; and, in the second place, constellated groups are far more easily described, named, remembered, and recognized. Absolute equality of interval, again, could only be thought of in connection with a series of single stars, and would be neither attainable nor likely to be insisted upon even then. If the moon's motion were quite equable, if she made her revolution in an even number of days, and never departed from the line of the ecliptic, we might conceive the impulse to look for equidistant single stars situated in the ecliptic to be of considerable force. But, in fact, there is the odd remnant of a part of a day more than twenty-seven, or less than twenty-eight, which breaks up the apparent regularity of the moon's nightly movement, so that in any revolution she is at a given time of night several degrees distant from where she was at the same time in the preceding revolution; moreover, her daily rate of motion varies quite notably, and this variation is cumulative, so that in one part of her revolution she is six or seven degrees behind, and, in another part, as much in advance of her mean place: nor are this retardation and acceleration, as in the case of the sun, constant in position, always taking place sensibly in the same part of the heavens; on the contrary, as her line of apsides revolves once in a little less than nine years, the variations of motion are rapidly shifting their action, and the moon may be, in parts of her course, a whole asterism in advance of or behind the position she occupied in her revolution four and a half years before, when of precisely the same sidereal age, or just as long after passing the initial point of the sphere. Again, the moon revolves, not in the ecliptic, but in an orbit which is inclined to that circle a little more than five degrees, and the line of her nodes makes the circuit of the heavens once in about eighteen years: so that if, at any time, a line of stars had been selected just upon her path, she would pass them, nine years later, at distances ranging all the way up to ten degrees. Finally, we must not leave out of account the fact that, during a considerable part of each revolution, the brilliancy of the moon's light is such as to obliterate entirely all but the brighter stars with which she comes closely in contact or near to which she passes, and the fainter ones at a still greater distance; so that to mark her movement by such stars only as are to be found immediately along the ecliptic would be unpractical; they could seldom be seen when she was close upon them, and often not when she was still at a distance of one or two asterisms to the eastward or westward. All the conditions, then, which would postulate a choice of single stars, or of stars or groups separated by precisely equal intervals, or confined to the immediate vicinity of the ecliptic, are so entirely wanting, that no *à priori* probability of the construction of such a series can be claimed; its existence in any nation could only be established by direct and unequivocal evidence—which has been shown above to be undiscovered as yet on Indian ground.

My view of the Hindu asterisms is accordingly in nearly all essential respects the same with that expressed in the notes on the Sûrya-Siddhânta: that they are, from the very beginning of their traceable history in India, a series of groups of stars, so prevailingly correspondent with those which are laid down by the later astronomical text-books as to be, notwithstanding one or two more or less distinctly traceable alterations, identically the same system; that they were looked upon as having been selected, for their number, their distribution, and their nearness to the moon's track, in order to mark the progress of that luminary along or near the ecliptic, distinguishing the portions of the heavens which she successively traversed; that under them the planetary path was regarded as subdivided into twenty-seven equal portions, yet by the eye alone, and without any of that precision and fixedness which are given by the habit of observing with astronomical instruments; that when, finally, a new and more exact astronomy had been brought in from the West, when the moon had been reduced in significance to one of a class of planetary bodies, all whose movements were capable of being predicted, and their places at any given time determined; when their conjunctions with one another had been made the subject of astrological speculation, and the calculation of them taught by an elaborate system of rules—that then the need was felt of treating in the same manner these star-groups, which had so long been the object of the Hindu astronomers' peculiar attention and veneration; that thereupon a selection was made of one star in each group, to represent the group in the calculation of conjunction, and hence to be called its junction-star; and that the position of the stars chosen was in such manner defined by astronomical coördinates as to render the calculation possible. The time at which this definition was made is the one date which is more certain than any other in the history of Hindu astronomy; the evidence presented at the end of our note to verses 2–9 of the eighth chapter of the Sûrya-Siddhânta will satisfy any one that it must have been not very far from A. D. 500. If there was any earlier definition, reckoned from another equinox, for which this one, recorded with unimportant variations in all the astronomical text-books, is a later substitution, no trace of it is known to have been discovered; nothing gives us reason to believe that it ever existed.

I shall now proceed to answer in detail some of the objections which M. Biot urges against this view—or rather, against the view which he assumed to be held by those Indianists who did not implicitly adopt his theory: for I do not find that he comprehended, as certainly he did not attempt to state, precisely what was believed by those who did not believe with him. That it implies, as he repeatedly asserts, any mode of astronomical observation which the ancient Hindus cannot be supposed to have known and put in practice, is palpably not the case; it contemplates no mode of study of the heavens which is not both possible and natural to all nations who have eyes and the power and disposition to direct them toward the heavens. Foothold can be won for this objection only by an arbitrary and impossible transfer of that definition of positions which was demonstrably made five centuries after our era to the earliest period of the history of the institution in India. That the Hindus of the time of the Brâhmaṇas did not deal in *vikshepa* and *dhruvaka*, or distance from the ecliptic on a circle of declination and distance of that circle on the ecliptic from the vernal equinox, will be readily granted. And yet it is by no means clear that they ought not to have done so upon M. Biot's theory. He holds that this peculiar system of coördinates is so distinctively Chinese in its character that it must have come directly from China into India, along with the "ancient *nakshatras*," in connection with which it appears in the eighth chapter of the Sûrya-Siddhânta; and he asserts that the restriction of its use to the *nakshatras*, and its inconsistency with the general modes of observation of the Hindus, employed in other cases, are plain proofs of its foreign origin. We must believe, then, that the tradition of this method of definition was religiously retained in India through all the long centuries during which the Hindus never employed it, and did not even preserve the data which it had originally yielded; and that, five centuries after Christ, when they had received from another quarter a complete astronomical system, and had new means and modes of observation, they still could not prevail upon themselves to observe the *nakshatras* otherwise than in the original Chinese manner, when they had to determine their places anew to make them fit into the modern system! How very implausible, not to say untenable and absurd, this supposition is, need not be pointed out. But we are not left upon this point to the consideration of implausibilities alone. Biot's statements that the mode of definition in question is confined to the *nakshatras*, and that it differs from that usual in other cases, are so utterly the reverse of true, that I cannot at all understand how he could allow himself to put them forth. If he had taken the trouble to look at the very next passage in the Sûrya-Siddhânta (viii. 10–12) to that in which the junction-stars of the asterisms are located in the heavens, he would have found other fixed stars located in the same manner: they are but few, it is true; but that is because the Hindu astronomy was not curious to concern itself with any others. For, again, if he had glanced through the whole treatise, he would have seen that it contains no trace of any other and different method of obtaining and defining positions by actual observation. The planets, it is true, have not their places laid down in this way, but it is for the reason that their situation is never to be determined by observation; it is obtainable by calculation from the data given in the treatise, which does not contemplate such a lack of faith in the astronomical student as should send him to the heavens to test the correctness of his inspired text-book. But the general treatment of the two modes of ascertaining the positions of the heavenly bodies, and the nomenclature applied to them, are such as show that they were looked upon as concordant, and the nearest approach to identity which the nature of the case permitted. The subject of the astronomical coördinates and their nomenclature is peculiar enough to justify a few words of explanation here. For "longitude" and "latitude," for "right ascension" and "declination," strictly speaking, the Sûrya-Siddhânta and the other text-books of its class, so far as known to me, offer no equivalent terms. The right ascension of any point on the ecliptic, but only on the ecliptic, may be approximately determined according to the methods of the treatise, by the awkward datum of the time occupied by the sign in which the given point is situated in rising above the horizon "at Lankâ," i. e., at the equator (see Sûrya-Siddhânta, iii. 42–49, and notes): it is used only in order to obtain the oblique ascension of the same point, and to determine what point of the ecliptic, at a given moment, is upon the meridian or at the horizon. Declination (*apakrama*, 'removal' from the equator) is also found only for the ecliptic, which is called the "circle of declination" (*apakramamaṇḍala*, etc.). For the longitude of a planet, or its calculated position in the ecliptic, we have no term whatever, but only the name of the planet itself: thus, performing certain operations, we have as a result "the planet," i. e., its longitude; we subtract Mars from Jupiter, to find the distance between them; we add six signs to the moon, to find the opposite point in the ecliptic, etc., etc. The technical language comes nearest to offering a synonym for the term latitude: it employs the word *vikshepa*, 'throwing off, disjection,' to designate that removal of a planet from the ecliptic which we call latitude, and which, in the Hindu view, is the distance to which the planet is thrown off from its proper track, the "circle of declination," by the influence residing in its node. But so little is the true value of this datum, and its peculiar character as measured upon a secondary to the ecliptic, present to the Hindu's mind, that we are taught, in order to find a planet's "corrected declination" (or, as we should call it, its declination), to add this *vikshepa* directly to, or subtract it from, the *apakrama*, or the declination of the point on the ecliptic occupied by the planet in longitude, as if the two were homogeneous. With this inaccuracy corresponds the fact that one of the coördinates by which the positions of the junction-stars are defined, the distance from the ecliptic on a circle of declination, is also called simply *vikshepa:* it is not acknowledged to be in any such manner different from the actual latitude of the planets as to need to be called by a different name. The term employed to designate the other coördinate likewise implies its homogeneousness with the calculated longitude of the planets: it is styled *dhruva* or *dhruvaka*, 'fixed, immovable:' whereas, namely, the planets are in constant revolution, ever changing their situation in the ecliptic, the junction-stars have their single "fixed" place upon that circle, which, having been once stated, is thenceforth always employable in calculations involving the positions of the asterisms.* The conclusion which will be drawn from this exposition by every unprejudiced person is, I am confident, that the coördinates in question wear no aspect of a Chinese or other foreign origin, but, at most, only indicate by their character the style of observation by means of which the positions of the junction-stars were determined, showing them to have been found by means of a meridian-circle, or something of equivalent character. If, indeed, it could be proved that the Hindus were unable to devise and put in execution such a method of observation as this—a method so feasible and natural, and adapting itself so well to the processes of calculation taught in the third chapter of the Sûrya-Siddhânta—or that they could have learned it from no other than Chinese teachers, the ground taken by Biot might be less untenable; but this I cannot conceive to be possible: the Hindu astronomers of the third to the sixth centuries of our era, who had long been sitting at the feet of Greek science, were not dependent on a miraculously preserved tradition of Chinese methods for the means of fixing the places of their asterisms.

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* This explanation of the term *dhruva*, as designating the "polar longitude," or "apparent longitude," of a *nakshatra*, appears to me now so obviously and incontrovertibly the true one, that I can only wonder that it did not suggest itself to us when engaged upon the Sûrya-Siddhânta.

M. Biot involves himself and his readers in a maze of difficulties with regard to the interpretation of the now well-known statement made in the Jyotisha, the earliest of extant Hindu astronomical treatises, which fixes the position of the solstices at the beginning of Âçleshâ and the middle of Çravishṭhâ, because he will not open his eyes to see that the statement implies that division of the ecliptic into twenty-seven equal parts of which he has chosen to deny the antiquity. This implication admitted, the two points referred to are seen to be opposite to one another upon the ecliptic, and all difficulty vanishes.

The absence from the astronomical systems of the Siddhântas of any division of the ecliptic corresponding to the asterism Abhijit is still explained by Biot as owing to the disappearance of that division in the course of the tenth century, by the coincidence of the circles of declination of its junction-star and that of the next asterism. He takes no notice of the considerations which, in the notes to the Sûrya-Siddhânta, we had urged against the admissibility of such an explanation, and which I cannot but regard now, as then, as entirely conclusive of its erroneousness. If they needed any farther aid to make them so, they have since found it from Weber, who shows that the omission of Abhijit is much more frequent than its inclusion in the series, even back to the very earliest traceable period of the history of the *nakshatra* system in India.

I have thus endeavored to show that the rectification of M. Biot's misapprehensions of the *nakshatra* system deprives of all ground and value one main division of the evidence upon which he claims to have proved the non-originality of the system in India, and its derivation from the Chinese *sieu*. Those who believe with him, however, will claim that the strength of his position is not seriously shaken. He has still his stronghold of the Chinese astronomy; if he has proved, by historical and scientific argument, that the *sieu* are an institution of native growth, we shall be forced to the acknowledgment that, mediately or immediately, they must have been the originals of the *nakshatras.* It is necessary, then, to submit this second division of the argument to a brief examination, and to see whether it possesses the cogency and conclusiveness which is claimed for it. In the general discussion of the subject presented in the notes to the Sûrya-Siddhânta, we refrained from entering upon this question at all: such a course is now no longer open, since, although I have made no original investigations bearing upon the points in discussion, I have before me, on the one hand, Weber's critical objections, contained in the first part of his essay on the *nakshatras*, and, on the other hand, the new detailed exposition of the history of Chinese astronomy which was drawn up by M. Biot—partly, as he states, in answer to our expressed misgivings—and which he regarded as sufficient to satisfy our doubts: if these are nevertheless unremoved, it is incumbent upon us to state the grounds of our continued incredulity.

This portion of M. Biot's argumentation also divides itself into two parts: the archæological and historical, founded on Chinese literary documents and records; and the scientific, founded on calculation and the facts of astronomical science. To the former part, Weber, in the first of his two essays, has brought up elaborate critical objections, maintaining that the *sieu*, as a system of twenty-eight determinants, or fundamental stars, are not traceable in the Chinese literature farther back than Lü-pu-wey, about 250 B. C., and that, according to its ancient and original Chinese arrangement, the series begins, not with Mao or the Pleiades, as Biot everywhere reports it, but with Kio or Spica Virginis. Now, whether agreeing or disagreeing with the objector in single points and matters of detail, few philologists, I think, will be inclined to deny these two propositions: first, that Weber's criticisms are of no small force, and must, until refuted, be regarded as seriously damaging Biot's argument; and second, that the latter, in his new exposition of the Chinese astronomy, presented in his last series of articles, has furnished no satisfactory reply to them. Biot does not, indeed, attempt to meet and repel his adversary on the latter's own ground, nor was such a thing to be expected of him, considering his entirely different training and habit of mind. As Weber had declined to meet him in the field of mathematical argument, acknowledging himself unequal to the encounter, and opposing to him a simple disbelief in the possibility of proving historical facts by such evidence, so he conceived himself justified *à fortiori* in casting out as naught the doubts of a skeptical philologist, when weighed against his own positive scientific arguments, and the probabilities which to him seemed derivable from the general history of astronomical science in China. He accordingly presented anew the same view of that history which he had given twenty years earlier, with no new fact or argument of any importance, and was content to rest his case upon it. But one who looks with a less partial and interested eye upon the exposition will be slow to draw from it any confident conclusion in favor of the antiquity of the system of determinant stars. If the witness of the Cheu-li be ruled out, as of too questionable value to be relied upon, in view of the suspicious character and history of that work, Biot's whole argument is mainly reducible to this: the Chinese people are so fixed and unchangeable in their ways, they have so constantly made astronomy and the regulation of the calendar a matter of prime importance, even laying stress upon it as a political institution, and it is so interwoven with their immemorial religious rites and observances, that—the system of the *sieu*, so essential an element of their later science, must have been of primeval antiquity among them. Now this is a conclusion which the argument is palpably too weak to sustain: we may believe all that M. Biot claims in favor of the antiquity of astronomical observation among the Chinese, without allowing that their science started in full possession of all the methods and appliances which it is found to use in later times. It is nowhere made to appear that the scientific results reached, or the religious ceremonies described, are either dependent upon, or intimately connected with, the employment of a certain number and series of fundamental stars. This last is only an element in the methods of observation made use of, a practical detail, and I see not why it might not have been introduced at any time without derogation to the fixity of Chinese political and religious institutions. Biot claims that the series was expanded from twenty-four to twenty-eight about 1100 B. C., after having subsisted for twelve centuries in its simpler form. If the primeval and unchangeable institutions of China could bear such a shock as this, we might trust them to endure the still greater one of the introduction of the whole system at a later date than M. Biot thinks could be allowed. They were not so unalterable as not to yield to the powerful influence of European science, represented by the Jesuit missionaries: these learned and adroit men succeeded in introducing Western modes of observation, and revolutionizing the construction of the Chinese calendar, only a century or two ago; but M. Biot points out no revolution in rites and ceremonies as accompanying a change compared with which the adoption of a series of fundamental stars whereby to note meridian transits is as nothing. As regards the appearance of the *sieu* in the Shi-King and the Shu-King, I am entirely of the opinion of Weber: that the mention in those works of stars or constellations which in part bear the same names with some of the defining-stars of the later system, and in part are identified with others of them by the commentators, does not in the least prove the subsistence of the system at the time; it only proves that the Chinese, industrious observers of the heavens as they seem to have been from a very early period, had already noticed and named some or all of the stars and constellations in the neighborhood of the ecliptic which are afterwards found to form a part of the series of the *sieu.* The exhortation of the mythical emperor Yao to his court-astronomers to walk forth in four different directions, and observe the equinoxes and solstices, is too obviously and grossly apocryphal to be seriously regarded. I do not, indeed, find of much account the difficulty which Weber puts in the front rank: namely, that the stars indicated as to be observed for the fixation of the points in question are not the stars at which those points are severally situated, but lie in each case ninety degrees eastward of them: we may, I should think, admit without scruple the explanation of the commentators, that the four stars mentioned are to be observed in the meridian when the point which each should mark is in the horizon, at sunset: we cannot, indeed, well suppose that the author of the imperial mandate meant anything else: but that the sage emperor should have overlooked the two important facts that meridian transits are not observable when the sun is on the horizon (unless with the aid of a telescope, of which he says nothing), and that the stars mentioned would not in all the four cases be upon the meridian at the setting of the sun in the equinoxes and solstices, is much less likely than that the later historian who put the words into his mouth should have been oblivious of them. The only conclusion derivable with any confidence from the record is that tradition or retrospective calculation had fixed with approximative correctness the positions of the equinoxes and solstices at the period assigned as that of the reign of Yao.

If the documentary part of M. Biot's argument is thus found unsatisfactory and inconclusive, let us see how the case stands with the scientific part. And here I would first remark that Prof. Weber, in my opinion, was more distrustful of his ability and right to enter as a critic into this department also, than he needed to be. There is no such peculiar character attaching to astronomical evidence upon historical points as should deny to a man of sense, learning, and critical judgment, the capacity of forming a competent opinion upon its cogency. It is not for the philologist to dispute the methods and formulas of the astronomer, and assume to reject his actual scientific results; but he may be as well qualified as the other, in a host of cases, and he is likely often to be much better qualified, to decide what is proved and what is not proved by the results attained. Few fallacies are of more frequent application, or have wrought more mischief, than that expressed in the proverb "figures cannot lie:" in a certain sense it is true enough; but there is no falsehood or absurdity so great that it may not be seemingly backed up and supported by a great deal of multiplication and division of irreproachable accuracy. And it does the highest credit to the candor and fairness of M. Biot that he has set forth with such admirable clearness the whole ground-work and detail of his astronomical evidence as to put it in the power of persons not possessing the thousandth part of his scientific knowledge to follow the steps of his argument, and form an independent judgment respecting their connection and tendency. He has, in the first place, determined, by means entirely within his competency, that at the period to which Cheu-kong is assigned, or 1100 B. C., the equinoxes and solstices occupied certain positions in the ecliptic, nearly coinciding with the determinants of the four *sieu* named Oei (35 Arietis), Lieu (δ Hydræ), Ti (α Libræ), and Nü (ε Aquarii), their distances from those stars ranging only from one-third of a degree to a little over three degrees. When, however, he goes on to claim that Cheu-kong, on account of the coincidence thus pointed out, added these four stars to the system of the *sieu* as it existed before his time, thus raising it from twenty-four to twenty-eight members, we who are unversed in astronomy have a right to sit in judgment on the inference, and to pronounce it fairly or unfairly drawn. To me, I must confess, it seems a good deal more than doubtful. Were there a well-attested Chinese record or tradition of the fact of the addition, we should accept M. Biot's calculation as a satisfactory and sufficient confirmation of it. But nothing of the kind is to be found. That the later Chinese, after the Christian era, report Cheu-kong as having observed the winter solstice in the second degree of Nü, is nothing to the point. Supposing him actually to have made the observation reported, the fact would not even prove that he knew the system of *sieu*, since the later astronomers, to whom the system was familiar, would naturally state the observation in that way, however it might have been originally described; far less would it be any indication that he went on to increase in consequence the number of the *sieu:* on the contrary, we might reasonably expect to find, along with the tradition of the observation, the tradition of so important a change resulting from it. That, in a series of twenty-eight stars equably distributed about the contour of the heavens, four should be at a distance of about ninety degrees from each other, so that, when a solstice pretty closely coincided with one of them, the opposite solstice and the equinoxes should nearly approach the others, does not seem so very strange as to forbid us to believe that all the four might have been old constituents of the series. If the origin of the system is that which all those who disagree in opinion with M. Biot claim it to be, the groups including these four Chinese determinants are entirely in place, and could not well have been passed over in making the selection of the asterisms. This is especially true of Ti, the southernmost of the two bright stars α and β Libræ, the most conspicuous constellation situated upon or near the ecliptic between Spica and Antares. If M. Biot's explanation of the selection of the other twenty-four determinants rested upon an impregnable foundation, and absolutely excluded these four, so that we were compelled to cast about us to see where they could have come from, the suggestion that Cheu-kong put them in for the reason assigned would be very welcome; but, in the absence of other corroborating evidence or favoring probabilities, we are both authorized and required to demand clear and unequivocal testimony from this quarter. The twenty-four stars constituting the original system are by M. Biot, as is well known to all who have had occasion to pay any attention to this subject, held to have been selected by the Chinese about 2357 B. C., upon two grounds: their proximity to the equator of the period, and the near correspondence of their circles of declination with those of the principal circumpolar stars. The Chinese of a still earlier period, Biot would have us believe, had been in the habit of particularly observing the circumpolar stars, of noting their transits across the meridian, and of comparing therewith the transits of other stars. In the gradual improvement of their processes, they hit upon the plan of taking their fundamental stars nearer to the equator, for the sake of greater facility and accuracy of observation; but they were still so far under the dominion of their former method that they made choice of such new stars as were virtual representatives of the old ones, standing upon nearly the same circles of declination. It is here, again, first to be noted that all this is pure hypothesis on the part of M. Biot, and not in the least founded upon any record or tradition in the Chinese literature. No Chinese author has been shown to attempt to give the time or the manner of the first establishment of the series of *sieu*, or to explain the motives of their selection. Hence, as before, we cannot help demanding a higher degree of force and cogency in the internal evidence than if it were brought in merely as auxiliary to documentary evidence. It needs more unequivocal proof to establish a theory fabricated in Paris in A. D. 1840 as to the reasons of a work performed in Lo-yang more than four thousand years ago, than would suffice to give credibility to an account of the matter which those who performed the work appeared to have themselves handed down to the after world. And, for my own part, having begun to examine the alleged proofs with a prepossession in their favor, and having been long conscious of a struggle against my doubts and misgivings, I must say that I now consider them totally insufficient. In the first place, as regards the proximity of the determinants to the equator: one has only to look at the table of coördinates of the whole system for B. C. 2357, given in Biot's first series of articles and repeated in his last series, and to cast his eye down along the column of declinations, to be startled by meeting with distances from the equator rising as high as over twenty degrees. In fact, the average of the declination of the determinants is nearly nine degrees (8° 52′), while that of their latitude, or distance from the ecliptic, is only a little over ten degrees (10° 12′). This difference is obviously too small to serve as the foundation of any convincing argument for their selection with reference to the equator, especially when the different requirements in the two cases are considered; those who had to choose along the fixed line of the equinoctial circle, and were willing to go as low as stars of the fifth magnitude in their choice, should have managed to attain a very much nearer average vicinity than those who, as has been pointed out above, had to look for conspicuous groups, and did not feel bound to the immediate neighborhood of the ecliptic. Moreover, if for the three stars (α and v^1 Hydræ, and α Crateris) which are peculiar to the Chinese system, and apparently variations from the original, be substituted those which there is good reason to regard as having belonged to the latter (α, ϑ, and β Leonis), the relation is inverted; the principal stars of the original asterisms average nearer to the ecliptic than do the Chinese *sieu* to the equator of B. C. 2357. Even the Hindu junction-stars, despite the introduction among them of such remote stars as those of the Lyre, the Eagle, and the Dolphin, average but twelve degrees and a half from the ecliptic. A like conclusion may be drawn from a simple ocular examination of our stellar chart showing the positions and relations of the three systems of asterisms, given in the notes to the Sûrya-Siddhânta, and upon which the position of the equator of B. C. 2357 is accurately laid down. The first three members of the series, indeed, follow quite closely that circle; but the next three still more notably disregard it. With the seventeenth member, again, begins a line of stars following a course of its own, far southward of the equator, which it does not approach at all nearly until the twenty-fourth *sieu* is reached; from that point to the end, the coincidence is tolerably close. The whole strength of Biot's argument evidently lies in the position of the eighth, ninth, and tenth members, which are those referred to above as being apparent deviations from the original series: were it not for these three, I greatly doubt whether M. Biot himself would ever have thought of the equator of B. C. 2357 as a factor in his hypothesis. But they are not enough to give effective support to so momentous a conclusion, especially considering their doubtful character, and the possibility and plausibility of the conjecture that they may have been substituted for the more ancient asterisms by the Chinese, at a vastly later date than B. C. 2357, for reasons of their own—as on account of their forming a more natural line of transition from the seventh *sieu* to the eleventh, and approaching much more nearly to the equator of that later time also when a borrowed system of lunar asterisms was converted into a series of right-ascensional fundamental stars.

We come next to consider the other motive of selection, the correspondence of the *sieu* in right ascension with the circumpolar stars. The best way, doubtless, to test and judge the validity of M. Biot's conclusions upon this point will be to examine in detail a sample of his reasonings. They are given in one of the tables* which form part of his earliest series of articles. He begins with the division Hiü, marked by β Aquarii, for the reason that it is nearest to the winter solstice, the point which the Chinese have long been most solicitous to fix, and upon the determination of which they have based their calendar. The division Hiü comprehends ten degrees of right ascension; its determinative does not at all closely agree with the solstice, but is (as is also pointed out in Biot's last articles) nearly seven degrees from it: the division also includes the inferior transits of the two bright circumpolars γ and δ Ursæ Majoris, but its limits by no means coincide with them; the one is six and a half, and the other seven and a half degrees distant from its commencement. If any ground is here to be discovered for the selection of β Aquarii as the limiting star of the *sieu*—it having at the same time nearly fifteen degrees of south declination—I, at least, am entirely unable to find it. The determinant of the next station, Goei, is α Aquarii; it is nearly three degrees east of the circle of declination of δ Ursæ Majoris, which we are taught to regard as having determined its selection, and its southern declination is thirteen degrees; the station extends almost nineteen degrees, to the circle of ε Ursæ Majoris. This is held to have fixed the choice of the next determinant, Che, or α Pegasi, and the coincidence of the two stars in right ascension is this time as close as could be desired. Biot brings also into connection with Che the inferior passages of 42 and 184 Draconis; one hardly sees why, the stars being so small as to be almost invisible to the naked eye, distant two and five degrees respectively in right ascension from the determinant, and having less than two degrees of polar distance. The limiting star of the next *sieu*, Py, is γ Pegasi; it was chosen, we are to believe, on account of its relation to the superior transit of β Ursæ Minoris, and the inferior of ζ Ursæ Majoris. The interval between these two transits is six degrees, and is nearly halved by the circle of declination of the determinant, which is two degrees from the one, and four from the other. Here, again, the plausibility of the argument is of the very faintest character: if the relation of the determinant to the circumpolars is to be one of such laxity and variability, if the circle of declination of the former is sometimes to coincide nearly with that of one of the latter, sometimes to fall midway between two of them, and sometimes to be so fixed that the *sieu* shall contain them, it is obviously easy to find reasons for the selection of a good part of any possible system of limiting stars; there will only now and then present itself an unmanageable case, resisting all attempts at explanation. Such a one is very near occurring at this point. For M. Biot is not a little doubtful as to how he is to account for the choice of ζ Andromedæ as determinant of the next station, Koei, and the consequent limitation of the station Py to nine degrees. The suggestion that it was for the purpose of shutting up the passages of β and ζ, above referred to, within a narrow space, fails to satisfy his mind, and with the best reason. He is also not content with the agreement of ζ Andromedæ in right ascension, within two degrees, with 5*a* Ursæ Minoris. On the whole, he thinks it most likely that the intention was to mark the point situated forty-five degrees from the solstice, a point which is hit within two degrees and two thirds by the selection of the limiting star Koei. The next determinant, β Arietis, introducing the mansion Leu, actually agrees very closely in right ascension with α Ursæ Minoris, our present pole-star, but then twenty-five degrees from the pole: yet M. Biot hardly seems to lay so much stress on this coincidence as on the fact that 10*i* Draconis, a star of the fifth magnitude, and less than two degrees from the pole, had crossed the meridian a little more than three degrees earlier. The following station, Oei, is marked by 35 Arietis, or *a* Muscæ, which has no relation to any circumpolars, and is regarded by M. Biot as added to the system by Cheu-kong, 1100 B. C., in the manner already related. The mansion Mao has as its determinative η Tauri, or Alcyone, the most brilliant of the Pleiads, which nearly marks the equinox of B. C. 2357, and is therefore made by Biot —as Weber maintains, without any support from his Chinese authorities—the commencement of the series. The farther limit of the mansion, ε Tauri, was fixed, we are told, so as to "include" the inferior transit of α Draconis; and the more brilliant Hyad, Aldebaran, was neglected because it was four degrees farther eastward, and so much more distant from the transit in question. But between the circles of declination of α Draconis and ε Tauri itself is an interval of four and a third degrees, so that δ or γ, we should think, ought to have been taken instead of ε, either of them answering better the required purpose. If it be objected that α Draconis was but two and a half degrees from the pole, and that hence the Chinese might easily have blundered a few degrees in referring its transit to the equator, I should willingly admit the force of the objection, but should claim, in addition, that it might also have excused the selection of Aldebaran itself; and farther, that this whole supposition of close observations made by the Chinese, twenty-five hundred years before Christ, upon the transits of stars situated but a degree or two from the pole, and of their determination of equatorial stations thereby, is void of any tolerable plausibility. The length of the station Pi, marked by ε Tauri, is eighteen degrees, owing, Biot thinks, to the absence of any remarkable transit of circumpolars. Next we come to a perfect nest of difficulties. We have two narrow stations, Tse and Tsan, which together occupy only a little more than six degrees of right ascension, followed by a third, Tsing, which includes the great space of over thirty degrees. The determinant of the first mansion, λ Orionis, is pretty near the equator, but that of the second, δ Orionis, is thirteen and a half degrees to the south of that line, and that of the third, μ Geminorum, is more than twelve degrees in the other direction from it. Here, one would think, is an opportunity for our astronomer's mode of explanation to display its power and value: if it can furnish a satisfactory account of so anomalous a condition of things as this, we can hardly avoid acknowledging that it is well-founded, and worthy of credence. But it can do no such thing; it breaks down entirely, and has not a single reasonable word to say for itself. The only circumpolar transit which has any relation to the transits of the three limiting stars Tse, Tsan, and Tsing, is that of κ Draconis, of the third magnitude, and eight degrees from the pole, and M. Biot, with the frankness and good faith which one cannot but constantly admire throughout his whole exposition, confesses that it has in the ancient Chinese catalogues no distinctive appellation, showing that any particular attention was ever paid it. Nevertheless, he thinks the mansion Tse, of less than three degrees, may have been established to "include" it—it makes its transit within half a degree of the termination of the station—and that the mansion Tsan, of about three and a half degrees, may have been established to "include" its transit at a somewhat earlier period, when its circle of declination reached the equator farther eastward! As to the choice of μ Geminorum as limiting star of Tsing, M. Biot can produce no reason which justifies it, and he also records his wonder at the selection of the two preceding determinants, when brilliant stars in Orion were to be had for the asking. This scantiness of circumpolar relations as justification of the three crowded determinatives Tse, Tsan, and Tsing, is set in still stronger light by contrast with the one next following, ϑ Cancri. Any good reason for the choice of this particular star as western limit of the mansion Kuei M. Biot finds it "impossible to conceive," since the magnitude of the star is but fifth to sixth, so that it is hardly discernible by the naked eye, and since it is also twenty degrees and a half from the equator of B.C. 2357; but the position of its circle of declination, and the extreme length of the station Tsing, he regards as altogether justified by reference to the superior transits of α and β Ursæ Majoris, which had to be waited for before the station could be closed. But, by his own account, the interval in right ascension between these two stars is more than eleven and a half degrees, and the circle of declination of the limiting star of Kuei, in order to apply to them both, is compelled to fall nearly midway between them, or about seven degrees to the eastward of the one, and five degrees to the westward of the other. We see, then, that, in order to save the credit of M. Biot's hypothesis, we shall be obliged to allow that the faint and undistinguished star κ Draconis could give locality to two or three determinatives, and fix the limits of as many mansions, while the brilliant α and β of the Great Bear, two of the most conspicuous of the circumpolars, and differing near twelve degrees in right ascension, could be represented at the equator by but a single star, and that one nearly invisible, and over twenty degrees from the equator! I am persuaded that the majority of unprejudiced critics will think, with me, that a theory which can only be retained at the cost of such violent assumptions as this had better itself be abandoned. But it must not fail to be noted farther that the circle of declination of β Ursæ Majoris is within less than two degrees of that of δ Hydræ, the determinative of the next station, Lieu, and their agreement is pointed out by M. Biot, and left to be alternatively regarded as the ground of selection of the latter star: he forgetting for the time that this is one of the four which he had already shown to have been added to the system by Cheu-kong, more than twelve centuries later. So that the determinant of the station Lieu, which had once been "proved by scientific evidence" to be of Cheu-kong's choice, is here exhibited as having had a notably better claim to selection by Yao himself than was possessed upon the average by the twenty-four stars actually singled out by that sage emperor.

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* "Table of the relations which have existed between the meridian passages of the twenty-eight stellar divisions of the Chinese, and the passages, superior or inferior, of the circumpolar stars, in the proleptic Julian year -2357, the presumed epoch of the emperor Yao, under the parallel of 34° to 40°." Journal des Savants, April, 1840, pp. 246-254; pp. 74-82 of the separate impression.

It is unnecessary to push our examination farther. We should meet, perhaps, with nothing quite so tellingly and unmistakably subversive of M. Biot's theory as that which is furnished us by the discussion of its application to these last few members of the system of *sieu*, but we should also meet with nothing more unequivocal in its favor. He who, after a careful survey of the whole exposition, can think that we have "positive scientific evidence" to the effect that the emperor Yao selected twenty-four of the twenty-eight *sieu* in the twenty-fourth century before Christ, and that Cheu-kong added the other four, thirteen centuries later, must estimate in a very peculiar manner the nature of scientific evidence and its application to the solution of historical questions. I do not hesitate to express my utter want of faith in the whole argument. In my view, what M. Biot has done may fairly be described as follows: he has reduced the *sieu* from twenty-eight to twenty-four by an arbitrary excision, and relegation to a later period, of four of their number; he has set up a list of nineteen circumpolar stars, whose upper and lower transits he assumes to have been observed with special care by the ancient Chinese, although he confesses that in some cases he can find no documentary evidence of the fact, and although several of them are so close to the pole that their observation in such ancient times, with such means as could be then applied, is in the highest degree questionable: these nineteen stars give him thirty-eight transits: he has then forced the twenty-four limiting-stars into an artificial and imaginary relation to the thirty-eight transits, by allowing the former to have been established, sometimes for the purpose of coinciding with, sometimes for the purpose of including, the latter; leaving, after all, some of the most important transits unrepresented by *sieu*, and having to confess that some of the *sieu* find no sufficient explanation in the transits. There are, indeed, a few rather curious and striking coincidences brought out by the comparison, and these, beyond all question, suggested to M. Biot his ingenious and captivating hypothesis; but they are no more than may with entire plausibility be supposed the result of chance, and are utterly insufficient to convert the hypothesis into an acceptable and credible explanation.

But if M. Biot's attempt to establish and account for the origin of the series of *sieu* in its present form at the remote date of the emperor Yao be deemed a failure, and if the essential identity of the Chinese *sieu*, the Hindu *nakshatras*, and the Arab *manâzil*, be conceded—a concession to which, I should think, no objection of any significance can be opposed—then it cannot be reasonably doubted that the system originally had that form and intent which we find it still to possess in Arabia and India: it was a series of stellar groups equably distributed along the ecliptic, and selected for the purpose of approximately marking the daily progress of the moon—thus constituting to all intents and purposes a true lunar zodiac; which, however, like the soli-lunar zodiac of twelve signs, was equally applicable, and more or less from an early time applied, to observation of the movements of the other planets, and to all the purposes of a general stellar division of the heavens. The question still admits of being mooted, by those who are jealous for the honor of China as the first home of the system, whether the Chinese may not have devised it and communicated it to other peoples in this its original form: but there can hardly remain a doubt that the shape which it now wears in China is the result of an alteration, made some time later than its origination or its reception from abroad. Evidence may perhaps yet be found in the Chinese authorities, to the effect that the names of the *sieu* were first used to indicate constellations rather than single stars. Biot himself gives us a valuable hint in this direction. At the foot of his second table, in the article of April 1840 (pp. 72-3 of the separate impression), he gives the meaning of some of the *sieu* names, and nearly all of them would fit groups better than single stars, while one case is a very plain one: Pi, the name of the second determinant, ε Tauri, he tells us, means 'a thread or string' (*le filet*), "which is the figurative designation of the Hyades." In view of the Indian and Arabian aspects of the system, it would be, I should think, very dangerous to assume that, when we find in an early Chinese author the name of a *sieu*, only the single star which the later astronomers know by that name can be meant, or even that the division of the heavens, where one is implied, is to be reckoned from star to star, and not, as in the other two systems, by simple proximity to the group named.

It deserves to be farther remarked, that the conversion of a series of groups into one of single stars used as determinatives or fundamental stars is vastly easier to explain than the reverse.* The former would naturally accompany the development of the Chinese astronomy into a more scientific form, and the introduction of new and more exact modes of observation. We should only have to suppose that the Chinese, after having for a time observed the movements of the moon and other planets by the eye alone or chiefly, and with reference to the asterismal groups, so far perfected their methods and appliances as to employ the meridian circle or its equivalent, with accompanying measurement of time; this would evidently throw out of use the old lunar zodiac, and might easily lead to the substitution of the modern limiting stars; which, in order to the least possible departure from ancient customs, were chosen out of the former and familiar groups. Such a change is closely analogous to what might have taken place also in India, if the Hindus had been willing to forget the old astrological significance of their *nakshatras*, or if the new device of the *yoga* or momentary conjunction had so recommended itself to them as to crowd out and replace the former superstition; the junction-stars might then have assumed the significance of the older groups, and the latter have been lost from use, and finally from remembrance.

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* If, at the time of publication of the Sûrya-Siddhânta, I was inclined to a different opinion (see this Journal, vi. 349, or Sûrya-Siddhânta, p. 205), I have long since seen my error.

It is, of course, impossible to restore with certainty the primitive form of the system of lunar asterisms. We may, however, make a tolerable approximation toward such a restoration by comparing together the three best known representatives of the system, and assuming that, wherever two of them agree and the third only is found to differ, the latter has varied from the original, which has been preserved unchanged by the two others. This assumption has an evident plausibility, although it can by no means be relied on as infallible: it could be proved in a great degree fallacious or unreliable only by showing that one of the three systems had been derived from another through the medium of the third; and this, in my view, is neither demonstrated nor plausibly to be presumed. I present upon the opposite page the results of a comparison of the kind suggested, adding a statement of the cases in which either system appears to have varied from the original norm, and also, in brackets, the member of each group selected by the Chinese as limiting star of a *sieu*, in their conversion of the series into a scheme of fundamental stars.

Only one member of the series, it will be noted, requires to be marked as wholly doubtful; namely, the fourth: with respect to this the three forms of the system are irreconcilably discordant, although in a manner which seems to indicate that the original locality of the asterism was probably in Orion. We cannot, indeed, but wonder at the original construction or present condition of the system as regards its third, fourth, and fifth members. In locating the third in the head of Orion, the constructors appear plainly to have neglected a group which was offered them as the natural and most eligible successor of the Hyades; namely, β and ζ Tauri, the tips of the Bull's horns: it is, in my view, the only case in which we have reason to find serious fault with their selection. And the neglect by the Chinese of the brilliant Twins, properly constituting the fifth asterism, with their substitution of a star which chances to be contained in the group to which the Arabs have apparently shifted their preceding, or fourth, asterism, is quite puzzling. The primitive place of the twenty-sixth member is also less confidently determinable than were to be wished. I cannot but conjecture, however, that the Hindus may have shifted their asterism (Revatî) at this point to the ecliptic at the time when they changed the commencement of the series from Kṛttikâ to Açvinî, and gave such an immense cosmical importance to the new initial point of the sphere: they might well enough feel desirous to fix this grand starting-point to the sense by a star situated directly upon it or in its immediate vicinity.

Primitive and Modified Forms of the System of Lunar Asterisms.

Probable original constituents of the system.	Hindu variations.	Arab variations.	Chinese variations.
1. η Tauri (Alcyone) etc. Pleiades.			[η Tauri.]
2. α Tauri (Aldebaran) etc. Hyades.			[ε Tauri.]
3. λ, φ^1, φ^2 Orionis. Head of Orion.			[λ Orionis.]
4. ?	4. α Orionis.	4. η, μ Geminorum etc.	4. δ Orionis.
5. α, β Geminorum. Castor and Pollux.			5. μ Geminorum.
6. γ, δ Cancri etc. Belly of Crab.			[θ Cancri.]
7. δ Hydræ etc. Head of Hydra.		7. ξ Cancri, λ Leonis.	[δ Hydræ.]
8. α Leonis (Regulus) etc. Head and neck of Lion.			8. α Hydræ.
9. δ, ϑ Leonis. Rump of Lion.			9. υ^1 Hydræ.
10. β, 93 Leonis. Tail of Lion.			10. α Crateris.
11. γ Corvi etc. The Crow.		11. β, η, γ, δ, ε Virginis.	[γ Corvi.]
12. α Virginis. Spica.			
13. ι, κ, λ Virginis. Edge of Virgin's robe.	13. α Bootis. Arcturus.		[κ Virginis.]
14. α, β Libræ. Claws of Scorpion.			[α Libræ.]
15. β, δ, π Scorpionis.			[π Scorpionis.]
16. σ, α, τ Scorpionis. Antares etc.			[σ Scorpionis.]
17. λ, υ Scorpionis etc. Tail of Scorpion.			[μ Scorpionis.]
18. δ Sagittarii etc. Bow of Sagittary.			[γ Sagittarii.]
19. σ Sagittarii etc. Left shoulder of Sagittary.		19. Space near π Sagittarii.	[φ Sagittarii.]
20. α, β Capricorni. Horn of Goat.	20. α Lyræ (Vega) etc.		[β Capricorni.]
21. ι Aquarii etc. Left hand of Waterbearer.	21. α, β, γ Aquilæ.		[ι Aquarii.]
22. β Aquarii etc. Left shoulder of do.	22. α Delphini etc.		[β Aquarii.]
23. α Aquarii etc. Right arm of do.	23. λ Aquarii etc.		[α Aquarii.]
24. α, β Pegasi. West side of Square in Pegasus.			[α Pegasi.]
25. γ Pegasi, α Andromedæ. East side of do.			[γ Pegasi.]
26. ? Left arm and side of Andromeda?	26. ζ Piscium etc.	26. β Andromedæ etc.	26. ζ Andromedæ.
27. β, γ Arietis. Right horn of Ram.			[β Arietis.]
28. 35, 39, 41 Arietis. Musca.			[35 Arietis.]

The other Hindu variations—except the strange and problematical one to λ Aquarii, at the twenty-third station—are evidently made in order to bring in conspicuous constellations from the north, and they are the greatest violations which have anywhere been committed of the proper design and spirit of the system. The Arab *manâzil* show the fewest departures from the primitive form, and these are attempted improvements, inspired by the original governing idea, the selection of groups nearly bordering upon the ecliptic. The knotty and troublesome point of the third, fourth, and fifth members, the Arabs have dealt with more successfully than either of the other possessors of the series. Except at this point, the only Chinese variations consist in the transfer of the eighth, ninth, and tenth asterisms to a situation farther south—a transfer of which note has been taken, and a conjectural explanation offered, in an earlier paragraph.

We come now to consider more particularly the views of Prof. Weber. This distinguished scholar, not content with refusing belief to M. Biot's theory, that the Hindu *nakshatras* are an adaptation of the Chinese *sieu*, holds as demonstrable, and attempts to prove, that the *sieu*, along with the *manâzil* of the Arabs, are directly derived from the *nakshatras*—the other two peoples having separately imported from India, and applied to their own uses, with slight alterations, an institution of Hindu origin. While fully acknowledging the very great value of Prof. Weber's essays—a value which is in but a slight degree dependent upon their discussion of the question of origin; since, along with the criticism of the Chinese authorities, already referred to, they offer us an almost exhaustive collection of materials for the study of the aspects and applications of the Hindu system in the earliest period of its history: a service for which we cannot be too grateful—I yet am unable to agree with him respecting this particular point, or to admit the force and pertinence of the arguments upon which he founds his opinion. I shall accordingly proceed to set forth the grounds of my dissent.

The whole strength of Weber's position lies in this thesis: that the Hindu *nakshatra* system has been a greatly and variously changing one, passing through a series of diverse forms; and that the Chinese and Arab systems represent one of its latest developments. If this is not proved, nothing is proved, and Weber's reasonings leave the question of origin, as they found it, unsolved. But, far from regarding it as proved, I do not think that he has made out even a tolerable case in its favor.

In the first place, his whole discussion is underlaid, as has been already pointed out, by the assumption that the *nakshatras* have the same character and use which we see in the Chinese *sieu:* that is to say, that they are virtually single stars, serving to mark out in the heavens and give name to intervals which are reckoned from one star to the next. If such an assumption be allowed, it can hardly be denied that the Indian system has undergone essential variations; variations the more strange, as they are to be found not only in different authorities, but even in different parts of the same authority. The Sûrya-Siddhânta, in its second chapter, gives to the twenty-seven *nakshatras* twenty-seven equal portions of the ecliptic; if these portions were taken from one group to the next, they imply a series of *nakshatras* very different from those whose junction-stars are so minutely defined by the same Siddhânta in its eighth chapter. So also, if Garga and Brahmagupta have chosen to assign only to fifteen *nakshatras* equal twenty-seventh portions of the circle, while to six they give a portion and a half, and to six only a half portion, they must have recognized a still different series, which no one has yet pointed out, or is likely ever to find. The same may be said of Varâha-mihira's definition of the mode of the moon's entrance into a *nakshatra*—that, in some cases, the apparent junction precedes the actual, or the reverse: whatever this may mean, it is obviously inconsistent with any division of which the boundaries should be determined, in the manner assumed by Weber, by the familiar constellations which we call the *nakshatras*. And yet, both Brahmagupta and Varâha-mihira define the positions of the junction-stars in the same manner as the Sûrya-Siddhânta, and use the same names for the different members of the two conflicting systems. It is almost superfluous, after this, to go back to the Brâhmaṇas for evidence to the same effect. When, in their mock-legendary narratives, they relate how king Soma (the moon)—much to his disgust, and after repeated attempts to avoid the disagreeable necessity, even by the breach of a promise solemnly given—has to pledge himself henceforth to dwell equally in succession with all his wives, the *nakshatras*, instead of passing his time, as hitherto, with his favorite Rohiṇî alone, we must conclude, according to Weber, that the stars limiting and dividing the asterisms were, in the view of the Brâhmaṇas, disposed at precisely equal distances along the ecliptic—if, indeed, we are not compelled to go farther, and maintain that they were arranged at intervals varying inversely as the moon's varying velocity in different parts of her revolution, with a provision annexed for gradual modification, as the revolution of the line of apsides, and other less constant causes, occasioned a shifting of the regions of her fastest and slowest motion: whereas originally she was stationary in Rohiṇî, or else this constellation extended all the way around the heavens. A hard time the Hindu star-gazers must have had of it, in truth, selecting so many different systems of stars, without help from the telescope, to fit all their discordant systems of division, and keeping them in harmony with the ever-varying irregularities of the moon's motion. But the assumption of this mode of division has been shown above, as I believe, to be altogether baseless; to find support neither from the Hindu authorities, ancient or modern, nor from analogy, nor from *à priori* considerations: it is nothing but a misapprehension. If we take that view of the *nakshatra* system which regards it as a series of groups of stars, occupying or pointing out, instead of limiting and separating, the divided portions of the planetary path, all difficulties and inconsistencies disappear; we see clearly that groups somewhat unequally distributed do not imply an unequal division of the ecliptic, nor an equal division require the recognition of a series of stars at equal distances; and that it is possible for authorities who accept precisely the same stellar series to differ from one another in the details of the division to which it serves as basis.

This erroneous fundamental assumption being removed, with its necessary implication of a shifting and changing series of stars, nearly all the other evidences of alteration in the system, which Weber is able to adduce, are at once seen to be of a very trifling and inconclusive character. They are chiefly variations in the names of the *nakshatras*, differences as regards the divinities to whom they belong, differences respecting the number of stars composing the groups, and the like, found among the Hindu authorities, ancient and modern. But all such variations are, within certain limits, perfectly natural and allowable, and consistent with the maintenance of the integrity of the system; they may even come to be evidence of its unaltered identity, as in more than one instance I think they actually are. Varieties of name, surely, in a language so fertile of synonyms as is the Sanskrit, as well in its scientific as its literary vocabulary, are not to be taken as even *primâ facie* evidence of varieties of character. If the Sûrya-Siddhânta, for instance, has a dozen names apiece for the sun and moon, we need not thereby be made doubtful whether the same two luminaries were really intended by them all. Nor need we in the case of the *nakshatras*, unless some other ground of probability of variation is made out, or unless the varying names are so characteristic, and point so distinctly to different stars or groups, that the conclusion of their diverse application is forced upon us. I cannot see that this is so in a single instance, nor does Weber make the claim, or endeavor to show that any given name could be better explained by referring it to a constellation not comprised in the series. Differences in the divinities selected as regents of the groups are of even less consequence. It is undeniable that in the Brâhmaṇas we approach pretty near to the beginnings, whether by origination or importation, of the *nakshatra* system in India;

and as no particular reason can be made out for the selection of one deity rather than another as lord of a particular asterism, we may with every reason suppose that for a considerable time, at least, more or less discordance in the choice would be found.

Differences in the number of stars regarded as composing an asterism would be of more telling weight, if they were such as could not be readily explained by the character and surroundings of the group with which the asterism is identified. But, in almost every case, they are, I believe, so explainable, and were duly noticed and accounted for in the detailed discussion of the identification of the groups which we gave in the notes to the Sûrya-Siddhânta. Thus, for instance, if the Kṛttikâs, the Pleiads, are reckoned now as six and now as seven, the variation should cause us no difficulty, considering the uncertainty which other nations besides the Hindus have felt upon the same point. The Greeks acknowledged seven Pleiads, counting in a lost sister; and to the Germans the constellation is still *das Siebengestirn*, 'the seven-stars.' Whether a seventh member of the group was actually once visible, and has now faded from view by that slow process of change which is known to have affected the brilliancy of certain stars, or whether the number seven was always the result of a mis-count, induced by the fancy for a mystic instead of a simple number, is a question of very little significance. Nor need we be embarrassed by statements in Hindu legends to the effect that the Kṛttikâs have their permanent station in the east, or that they formerly accompanied the brilliant stars in the Great Bear: such statements, it is evident, are pure creations of fancy; from any astronomical point of view, they are simply nonsense. That the brilliant star Rohiṇî, or Aldebaran, was now taken by itself to form the asterism, and now along with the other members of the group of which it is the chief ornament, is very natural, and is no reason for inferring a change in the position of the asterism. That ζ Hydræ was sometimes added to the little group of five stars constituting the asterism Âçleshâ, and ζ or ε Delphini to the four Çravishṭhâs, is not less easy to believe. That, of the extensive constellation forming the tail of the Scorpion, now only the brilliant pair in the sting were made to compose the asterism Mûla, and now more or fewer of the others were included with them, is readily supposable, and does not render the identity of the asterism suspicious. And so with the few other like cases of variation which the system presents.

I should not always be willing to admit the force of the evidence on which Weber relies to prove a variation in the number of stars composing an asterism: in certain cases, namely, where it depends upon the grammatical number of the name of the asterism, and on variations therein. If a group has a plural name, indeed, it must, unless some good reason to the contrary can be shown, be regarded as composed of more than two stars; but this name may vary to singular, without by any means implying a reduction of the group to a single star: only its contemplation as a single group, as an individual member of the series of asterisms, or as one of the moon's consorts, may be implied. In this manner the Kṛttikâs may become Kṛttikâ, the Maghâs may become Maghâ, and so on. A dual name, again, is even more distinctly indicative of a pair of stars; and, as we should expect, wherever the nomenclature of the system presents us a dual name, there we find in the heavens a conspicuous pair to which to attach it: we have, for Açvayujâu, β and γ Arietis; for Punarvasû, α and β Geminorum; for Viçâkhe, α and β Libræ; for Viçṛtâu, λ and υ Scorpionis. These, too, may vary to singulars in the same manner as the plurals do, and we may find Punarvasu, Viçâkhâ, etc.; or they may even become plurals in consequence of an extension of the group by the inclusion of other neighboring stars. Another variation between dual and plural, or between dual, plural, and singular, has a different ground: it occurs in cases where a single group, having a collective name, is divided into two asterisms. Thus, the two asterisms in the rump and tail of the Lion form a single quadrate constellation, and are called *phalgunyas* or *arjunyas*, 'the shimmering or glowing ones;' but, as two members of the *nakshatra* system, they are also made dual, *phalgunyâu*, 'the two Phalgunîs,' precisely as other plurals have been shown to be converted to singulars; and each of the two also becomes by itself dual, as being composed of a pair of stars. A similar case is that of the asterisms which together compose the conspicuous group known as the Square in Pegasus: their usual titles, *proshṭhapadâs* and *bhadrapadâs* or *bhâdrapadâs*, contain the element *pada*, 'foot,' and the allusion conveyed in these names to the disposition of the stars in the constellation, like the four feet of an animal or of an article of furniture, is so marked and unequivocal that only extreme prepossession could induce any one to overlook it. Another rare appellation of the double group, the singular *pratishṭhâna*, 'stand, support,' is as plainly due to a like conception of its form. Both these names farther appear as dual, and even as double dual, for the same reason for which the name *phalgunî* was so treated. But the conclusion, in the third place, from a singular name that an asterism contains only a single star is vastly more doubtful, and even, in my view, palpably ungrounded, since such a title may from the beginning have designated the group as a group. Thus the name Mṛgaçiras, 'deer's head,' has nothing to say respecting the number of stars of which it may be the collective designation; while Hasta, 'hand,' may with evident probability be regarded as pointing out the very group of five stars to which we find it attached by the astronomical text-books. The adjective *ekanakshatra*, applied to some of the asterisms, obviously designates them, not as 'single-star *nakshatras*,' but as 'singular-name *nakshatras*:' just as *puṁnakshatra* does not mean 'male *nakshatra*,' but 'masculine-name *nakshatra*:' another and yet more singular instance of the weight attributed to petty peculiarities in the names of the groups is to be found in the division of them referred to by Weber (see note 2 on p. 311 of the first essay) as founded on the number of syllables in their respective titles.

The final conclusion, then, at which Weber arrives, that there are but four of the *nakshatras* with regard to the number of stars composing which there is an utter absence of discordance among the different authorities, although it looks quite startling, has in fact only an illusory force, since it is founded on and represents differences which are in part imaginary, and in part trivial. There are but one or two of this whole class of variations which need cause doubt or difficulty to any one: it seems strange, namely, that a single star out of so faint a group as that composing Pushya (γ, δ, ϑ Cancri), should have been by any authority regarded as alone constituting the asterism: it is also hard to see why the name of the asterism Ârdrâ (α Orionis) should by one or two authorities be given in the plural, and why its synonym *bâhu*, 'arm,' is once made dual, as if a group of two or more stars were intended to be pointed out. But, even in these cases, the sum of evidence and of probability in favor of the unchanged identity of the asterism is so strong that our faith in it need not be seriously shaken.

In more than one instance, if I do not misjudge, Weber's anterior persuasion of the shifting character of the *nakshatra* series, and his inclination to interpret everything in a manner favoring it, have misled him into very farfetched and unnatural hypotheses, to the neglect of such explanations as lay close at hand and possessed an evident plausibility. Let us note two or three examples. The brilliant star of the first magnitude in the head of Taurus, called by us Aldebaran, constitutes a member of the system of asterisms, either alone or with the other members of the group of Hyades: its name is *Rohiṇî*, which means literally 'red, ruddy.' As the star in question is one of the few which shine with a conspicuously reddish light, the supposition naturally suggests itself that this characteristic determined the appellation of the *nakshatra*, and that the appellation and the answering characteristic constitute together an important evidence of the unchanged identity of the asterism from the first institution of the system. Nothing of this, however, appears to Weber; he mentions, indeed, the possible interpretation of *rohiṇî* as 'red,' but does not allude to any natural reason for its application to the *nakshatra.* At another point in the series occurs another brilliant red star, our Antares, or Cor Scorpionis. This usually has the name Jyeshṭhâ, 'eldest,' but in two of the most ancient authorities it also is styled Rohiṇî, 'the ruddy.' Here, again, we can hardly avoid concluding, one would think, that this second asterism is called "the red" for the same reason as the former one, and we may even accept the synonym as a welcome confirmation of the persistency of the asterism. Yet Weber apparently is not satisfied with an explanation so simple. He, on his part, takes note of the fact that the two stars in question are one hundred and eighty degrees apart—as they actually happen to be, very nearly—and that the former of them is in the Hindu traditions regarded as the special favorite of the moon; and upon these facts he proceeds to found a conjecture (which he then, in the next sentence, gives up again, as too utterly implausible), that the reason why the moon once particularly loved Rohiṇî was that the latter coincided with the vernal equinox (which would have been the case more than thirty centuries before Christ), and that the other Rohiṇî was so called because, at the same period, it occupied the other equinox, and so headed the second half of the *nakshatras*, as the former Rohiṇî the first half! Does not this seem like a wilful closing of the eyes to a reason which lies near at hand, in order to make a long journey for the purpose of catching at one which must be immediately relinquished as worthless? But Weber offers us another instance, not less striking, of the same kind, in connection with this very asterism Rohiṇî. It so happens that the word *rohiṇî* is also the name of the female of a species of red-deer. The Hindus, moreover, figured to themselves the neighboring constellation Orion as a stag or antelope; this appears partly from the fact that the little group of three stars in Orion's head is called *mṛgaçiras*, 'the antelope's head;' partly, that his left shoulder (α Orionis) is sometimes styled, in the synonymy of the asterisms, *bâhu*, 'arm,' i. e., 'fore-leg;' and partly from the tradition which I shall presently relate. The somewhat remoter Sirius is also named in the astronomical text-books *mṛgavyâdha*, 'the deer-slayer.' Whether, now, the establishment of this whole series of related constellations with related titles grew out of the circumstance that *rohiṇî* was capable of being translated 'female red-deer,' along with the later-observed adaptedness of the stars composing Orion's figure to bear their part in the combination; or whether, as seems more probable, the recognition and naming of the "stag" was an independent fact, and its vicinity to the "red-deer" merely accidental, and perhaps even the occasion of the interpretation of the latter's name in that sense—is a question of trifling importance: the situation and connection of the three groups is at least measurably assured by their relation to each other, and is rendered altogether unquestionable by an absurd story which the Hindus have founded upon it, and which Weber cites in different forms, from more than one of the Brâhmaṇas. Prajâpati, 'the lord of created beings' (an unfortunate divinity, often called upon to play a part in these artificial legends, in the manufacture of which, to order and in batches, the Hindus of the Brâhmaṇa period were so apt), it is said, fell in love with his own daughter Rohiṇî; she becoming a female deer, he pursued her in the form of a stag, and was only deterred from his incestuous chase by being shot in the back with an arrow by the appointed agent of the indignant gods. There is the whole story illustrated in the sky: the innocent and lovely Rohiṇî (Aldebaran); the infamous Prajâpati (Orion) in full career after her, but laid sprawling by the "three-jointed arrow" (the belt of Orion), which, shot from the hand of the near avenger (Sirius), is even now to be seen sticking in his body. With this tale coming down to us from the first period of the *nakshatras* in India, who could suppose that, when we have to locate a member of the system called *mṛgaçiras*, 'the stag's head,' and have just this part of the sky to locate it in, there could be an instant's hesitation as to its position, or the least doubt of its persistent identity, from the earliest times to the latest? Yet Weber does not see it so: in his view, there is nothing which at all fixes the name *mṛgaçiras* to any particular constellation, nothing to prevent us from supposing that it was applied successively to various constellations, as one and another was selected for a boundary-mark in the ever varying divisions of the ecliptic: if the earliest authority giving the numbers of the groups assigns three stars to Mṛgaçiras, this, he thinks, is a mere inference from the name; a stag's head, with a horn on each side of it, naturally suggested the number three;* and if we find it in the most modern form of the system an actual and indubitable group of three stars, this, we must suppose, is mere chance; the good luck of those under whose hands the system received its latest development enabled them to realize in the heavens the etymological fancy of their remote predecessors! A similar suggestion is thrown out in regard to Çravaṇa. If this is reckoned as a group of three stars, it is not because in early times, as in later, the name really belonged to the conspicuous constellation of the Eagle, a star of the first magnitude with a smaller one above and another below, but because *çravaṇa* means 'ear,' and so led one to think of a pair of ears, with a head between them! Of this implausible, not to say impossible and absurd, character are the suppositions which we are called on to make in support of Weber's theory respecting the shifting and changing character of the *nakshatra* system.

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* "Bei *mṛgaçiras*, 'Rehkopf,' dachte man sich wohl der Hörner wegen die Dreizahl nöthig." p. 381.

At two points, however, Weber is able to bring forward what seems to him direct and positive evidence of a different position of the stellar groups composing *nakshatras* from that which is assigned them in the Siddhântas. In the first place, Abhijit, the problematical twenty-second member of the series of twenty-eight asterisms, is in the Tâittirîya Brâhmaṇa, the earliest authority which mentions it, described as situated "above the Ashâḍhâs, below Çroṇâ" (*uparishṭâd ashâḍhânâm, avastâc chroṇâyâi.* T. Br. I. v. 2. 3); and this is regarded by Weber as equivalent to assigning the group a place somewhere in Aquila, instead of Lyra. But I see no necessity for understanding the language of the Brâhmaṇa to define the stellar position of the group; I would consider it rather as fixing the order of the asterism in the series: Abhijit is, in the succession of the *nakshatras*, next above or beyond the Ashâḍhâs, and next below or this side of Çroṇâ (Çravaṇa). This interpretation is, in every aspect, quite as admissible as the other, and the citation from the Brâhmaṇa is hence without avail to show a change of place since its time in the asterism Abhijit. The other case is of a less disputable character. The same authority, the Tâittirîya Brâhmaṇa, in a passage immediately preceding the one already quoted, constructs a stellar *prajâpati*, giving him Citrâ (α Virginis) for head, Hasta (Corvus) for hand, the Viçâkhe (α and β Libræ) for thighs, and the Anurâdhâs (β, δ, and π Scorpionis) for standing-place; while Nishṭyâ (i. e. Svâti) is declared to be his heart. This would make a very tolerable figure, as constellational figures go, but for the heart, which, if Nishṭyâ must be sought in Arcturus, lies some thirty degrees north of its proper place; while, on the other hand, if it can be assigned the position given to the corresponding asterism in the Arab and Chinese systems (ι, κ, and λ Virginis), the difficulty is wholly removed. Hence Weber claims it as altogether probable that, in the time of the Brâhmaṇa, or to the apprehension of its authors, the asterism next succeeding Citrâ was still situated close upon the ecliptic, like the corresponding *manzil* and *sieu;* and the force of the argument and the legitimacy of the conclusion cannot be denied. Absolutely certain we could not venture to call it, since it is not quite inconceivable that the constructor of the figure may have been careless of the position of the heart, when all the other parts fitted so tolerably, or may have been more mindful of the order of Svâti, between Citrâ and Viçâkhe, than of the position of the stars constituting the asterism.

The preceding discussion has shown us that (setting aside the expansion of the system from twenty-seven to twenty-eight members, or its reduction from twenty-eight to twenty-seven, of which I shall presently speak) all the evidences marshalled by Weber to attest the variable character of the *nakshatra* series prove only this: that a single member of it must have had formerly in India the same position which we find it always to have had in Arabia and China. How untoward a result this is of an argument intended to demonstrate that the *manâzil* and *sieu* must have been derived from the *nakshatras,* as representing one of the latest phases of that variable system, does not require to be pointed out. We have seen reason above, upon a simple comparison of the three series of asterisms with one another, to infer that the Hindus must probably have altered theirs, since its origination or importation, in at least six members; which is far more than is established with the slightest shadow of plausibility by all Weber's doubts and questionings. Even this degree of alteration, affecting more than one fifth of the whole number of the *nakshatras*, does not by any means destroy or seriously impair their character as a fixed and stable system, or give them that Protean aspect which Weber would claim for them. I cannot but think that his view of what is required to constitute a system such as we see represented by the *nakshatras*, the *manâzil*, and the *sieu*, is fundamentally erroneous. He appears to suppose that a recognition of the fact that the moon makes the circuit of the heavens in twenty-seven or twenty-eight days, and that therefore her path is divisible into the same number of parts or stations, is itself the system, or the essential part of it, while its fixation in the sky, by selected stars or groups of stars, is a matter of secondary consequence. To my apprehension, on the contrary, this recognition is only the necessary preliminary to the establishment of a system. There is no such thing as a system of *nakshatras* until the series of stellar groups is determined upon, as guides to the actual division and nomenclature of the path of the planets; and its existence is bound to these groups, so that those who use and value it will not readily forget them, or hold them subject to arbitrary and sweeping alteration. It may be modified in one and another point, and even to no inconsiderable extent, but by gradual and successive changes, such as leave its identity and virtual integrity each time unimpaired. I have seen no evidence, in Weber's essays or elsewhere, which was of any avail to prove that the Hindu *nakshatras* did not possess this character; the name *nakshatra* itself, with its synonyms, which all mean without exception 'star, constellation, asterism,' is sufficient to show that the stellar groups were its central and prominent feature. Were we dealing with the Arab or Chinese systems, in both of which the name signifies 'mansions, stopping-places, stations,' there would be more plausibility in regarding the division as the primary matter, and the identity of the determinant groups as of subordinate consequence: although even here the view would be fallacious, since the divisions possess no recognized existence until they are determined by the groups: but a system of *nakshatras* is one of "asterisms," its members are stellar individualities, and it can have no stability or value if these are changeable at will. It is not necessary for me to set forth here in any express and detailed manner the positive evidence, showing the fixedness and permanency of the stellar groups composing the *nakshatras*, which is derivable from the accounts of the system given by earlier and later Hindu authorities: some of this evidence has already been presented incidentally; and, in view of the utter failure of the only attempt yet made to show the reverse, and the absence of any important opposing considerations, documentary or theoretical, it may be taken for granted, and the burden of proof thrown upon any one by whom it is denied. I will merely say that, in my opinion, the whole nomenclature of the system, the notices respecting the numbers and relations of the stars of its groups, the related legends, and the like, do in every case (excepting that of Svâti, mentioned above), and from the beginning, so far as they are distinctive at all, point to the same series of asterisms which we derive, with all the certainty and authority which a definition by astronomical coördinates confers, from the latest astronomical treatises.

There remains for examination a single point, more pregnant with import than any hitherto discussed, in its bearing on the question of change in the Hindu system, and the derivation from it of the other two. It is the number of constituent members of the *nakshatra* series: the question of the relation between the numbers twenty-seven and twenty-eight, both which are to be met with in Hindu authorities, ancient and modern. Weber holds that the groups were originally twenty-seven, and became twenty-eight at a later period, by the addition of Abhijit. If this be fully and satisfactorily proved, the Hindu origin of the Arab and Chinese systems will be hard to deny; since it must appear at least highly improbable—to call it altogether impossible I should hardly venture—that the Arabs and Chinese should on their part also, and independently, have expanded to twenty-eight an original series of twenty-seven. I have purposely reserved until now any mention or consideration of this point, in order that I might first remove all other supports from Weber's thesis, and show that, if we are to pronounce the Hindu system the parent of the others for the reason that they represent a later phase of its development, our decision must be based solely and alone upon the ground of the change of number. We shall hence be led to require that the evidence of the change be very clear and unambiguous; for a less degree of force and cogency will be tolerated in a reason which comes in as a cumulative addition at the end of a series of probable proofs, than will content us in an argument on which must repose the whole weight of an important conclusion. In my view, the argument is in this case too insecurely founded to sustain so heavy a burden; the evidence of the priority of the system of twenty-seven members is not sufficiently unequivocal for the purpose to which it is applied. If we found this system, with the twenty-seven-fold division of the ecliptic and all the other dependent features, uniformly and alone presented in the authorities preceding a certain date, and if we found later the system of twenty-eight, serving as foundation to a twenty-eight-fold division of the ecliptic, and to the other uses of such a system, crowding its predecessor out of use and usurping its place, then would the matter be a tolerably clear one: it could fairly be claimed that the Hindus first possessed a series of twenty-seven *nakshatras*, to which they afterwards added a twenty-eighth, and that the other nations borrowed it of them after this extension. But the case stands not so. It is true that in the earlier authorities the decidedly prevailing number is twenty-seven; but this is equally true also of the later authorities: down to the final fixation of the Hindu astronomy in its scientific form under Western influence, the *nakshatras*, for all the practical purposes of a series of asterisms determining portions of the ecliptic, are and remain only twenty-seven. The Sûrya-Siddhânta knows nothing of any division of the planetary path into twenty-eight parts, nor, according to its authority, could the moon or any other planet ever be present in the *nakshatra* Abhijit; they may all, indeed, enter into *yoga* or momentary conjunction with Abhijit; but so they may with one another, and with sundry of the fixed stars which never pretended to claim a right to be regarded as *nakshatras*. No one, so far as I know, has brought forward evidence to show that any Hindu authority has divided the "circle of the asterisms" into twenty-eight equal arcs, thus giving the larger system the same form and application as the smaller one. But, side by side with the recognition of twenty-seven asterisms as determining divisions of the ecliptic, sometimes in the same authority, sometimes in other authorities of the same period and character, is found a recognition of twenty-eight star-groups, as constituting, under certain aspects and for certain purposes, the complete series. So far as I can see, there is no time in the history of the system in India, at which any other nation borrowing it from the Hindus would not have been quite as likely, or more so, to receive it with twenty-seven constellations, as with twenty-eight. Let us next see how the case stands as to priority of mention. The twenty-seven are, as is shown by Weber, first spoken of in the Tâittirîya Sanhitâ: the twenty-eight make their first appearance in the Tâittirîya Brâhmaṇa and in the nineteenth book of the Atharva-Veda. That the former mention is probably the earliest of the three, no one will be disposed to deny; but he will be a bold man who ventures to assert that it must be very much earlier than the others. There is confessedly no such difference of period between the Sanhitâ and Brâhmaṇa of the Black Yajus as between those of the Rig-Veda. In both divisions of the former, as was long ago pointed out, by Weber and others, there is a complete intermingling of the two classes of material known as *mantra* and *brâhmaṇa*, and the Brâhmaṇa is the immediate continuation and complement of the Sanhitâ. All mention of the *nakshatras* which is made in either work is in parts of the text which would be ranked as *brâhmaṇa*, and which, if upon the whole of later origin in the Brâhmaṇa than in the Sanhitâ, yet all belong to the same period, and need not be separated by any considerable interval of time. Considering how the traditionary materials were gathered up and combined in the later Vedic age, it is by no means impossible that the Atharvan hymn should be as ancient as anything which mentions the complete series of asterisms.* The argument of prior mention, then, amounts merely to this: that, of the not numerous specifications of the number of members composing the *nakshatra* system which are to be found in the literature of the Brâhmaṇa period, the greater part, and among these the one or two which are presumably the most ancient, speak of twenty-seven only. Very nearly the same thing, as it happens, could be said of the more modern astronomical literature; the Jyotisha, which is confessedly more ancient than the works of the Siddhânta class, mentions only twenty-seven asterisms; and, if the yet earlier literature were swept away, we might draw the probable conclusion that the system was expanded from twenty-seven to twenty-eight after the time of the Jyotisha and before that of the Siddhântas: a conclusion which would be not a little fortified by the circumstance that an awkward attempt has been made to crowd Abhijit into the Jyotisha also, as if authority were sought for a later addition by its intrusion into a work whose existence preceded its own origin; while, as the case stands, we know that Abhijit as a *nakshatra* is much older than the Jyotisha, and was omitted from it only as it is also omitted from the second chapter of the Sûrya-Siddhânta—because it had no concern with the equable division of the ecliptic into portions. I am far from claiming that there is anything in the Hindu authorities which makes directly against the theory of the originality of the series of twenty-seven groups in India, and its later expansion into one of twenty-eight; on the contrary, I freely acknowledge that, upon Indian ground alone, that theory is rather more probable than the opposite; but I do contend, and with much confidence, that it is not forced upon us by the facts, and does not, to the exclusion of any other, furnish their only acceptable explanation. There is no insuperable, or even formidable, obstacle in the way of our supposing that, if the Hindus received the institution from abroad, they received it as a series of twenty-eight groups, but soon, for reasons of their own, reduced it to twenty-seven members; while yet the remembrance of the other form was never entirely effaced, and the rejected asterism appeared, to the last, in certain aspects and applications of the system; so that while, as a foundation for the division of the ecliptic, it always maintained, from the time of the first re-formation, its twenty-seven-fold character, it nevertheless, in what concerned the asterisms as groups of stars simply, was sometimes regarded as still possessing its twenty-eighth member. We are fully justified in setting aside as indecisive the argument from change of numbers, in refusing to believe, on this ground solely, that the *manâzil* and the *sieu* are derivative forms of the *nakshatras*, and in inquiring farther whether the question of origin is to be determined, in preference, by other considerations.

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* Weber finds in its nomenclature evidences of a later origin, but they seem to me far from unequivocal. The form *çroṇâ* for *çravaṇa* was very probably, as the metre shows, the original reading of the text; and that *mûlabarhaṇî*, *jyeshṭhaghṇî* are earlier names than *mûla* and *jyeshṭhâ* is what I am very unwilling to believe: they seem to me, rather, titles of reproach, manufactured out of the usual and proper appellations (Weber himself, with much plausibility, explains in a like manner the name *aghâs*, for Maghâs, in Rig-Veda x. 85. 15), and applied to these two asterisms in view of the unlucky astrological character which men had come to ascribe to them. No such stigmas are affixed elsewhere in the series, nor can we plausibly suppose the astrological element to have been predominant, or prominent, at the first invention or introduction of the *nakshatra* system.

I would here remark that the nearer coincidence of the moon's revolution with twenty-seven days than with twenty-eight does not seem to afford reason for concluding that the former number must have been earlier and more original than the latter, as belonging to the constellations selected to compose the lunar zodiac. So long as there was a fractional part of a day, more than twenty-seven and less than twenty-eight, entailing a discordance in the relations of the stellar groups to the moon's place in any two successive revolutions, it would be comparatively an indifferent matter whether the odd third of a day were disregarded altogether, or made to count as a whole member, notwithstanding its brevity. The choice would be likely to be determined by the character of the number which it afforded, as being conveniently divisible, as having attractive associations and suggestions, or the like. If eleven and one third synodical revolutions of the moon made up the period of the earth's revolution around the sun, instead of twelve and one third, as is actually the case, the year would doubtless none the less be reckoned as composed of twelve months, because of the arithmetical character of the number twelve, as divisible by two, three, four, and six, while eleven is a peculiarly impracticable number. As regards twenty-eight and twenty-seven, each has its advantages. The one is divisible by two and four, and in the latter case with the quotient seven; hence it would recommend itself to a people who made much of the number seven, and who had the week. On the other hand, one may conceive that the Hindus, knowing no such division of time as the week, and less regardful of the mystic significance of the number seven than their Semitic neighbors, should have been led to amend to twenty-seven, a number which had the recommendation of being a cube, by the practical consideration that the error was less if not twenty-eight, but twenty-seven stations were counted in the moon's revolution.

If the views of the earliest form of the system of lunar asterisms and its changes which have been set forth in an earlier part of this paper were well founded, we may find in them some slight confirmation of the originality of the number twenty-eight, and a facilitation of the Hindu reduction to twenty-seven. In the line of groups which stretches between the nineteenth and twenty-fourth members of the series (according to the ancient numbering), or between the Hindu double asterisms Ashâḍhâs and Bhâdrapadâs, in nearly uniform distribution and with nearly equal brilliancy, we are at a loss to know which should have been left out in an original system of twenty-seven asterisms; all seem to possess an equal right to a place. When, however, the Hindus had substituted for the first three of these the more conspicuous but remoter constellations of the Dolphin, the Eagle, and the Lyre, the last of them situated more than sixty degrees from the ecliptic, it would seem very easy, if the slightest desire for a reduction to twenty-seven members were felt, to leave out altogether a group which, by its position, had so little claim or fitness to point out and give name to a portion of the planetary path. I lay no stress on these considerations, but merely deem them worthy of a brief mention.

The results thus far won are of a negative character only. We have seen, on the one hand, that M. Biot's argument for the originality and immense antiquity of the Chinese *sieu*, and for their communication from China to the countries lying farther westward, is entirely nugatory; we have seen, on the other hand, that Prof. Weber's attempt to prove that the Hindu system is the parent, and the others its descendants, is not less a failure. The question of actual origin is still untouched. We have only cleared the ground; we have left the way open to prove, by good and sufficient evidence, that either one of the three nations referred to, or that some fourth people, different from them all, is entitled to the honor of being regarded as the inventor of an institution so widely diffused, and forming a cardinal element in the astronomical science of the most important and highly cultivated races of Asia. Into this general inquiry I do not propose to enter with any thoroughness; I only wish to ask here what reason there is to believe, or to suspect, that India is not the original home of the asterismal system. In entertaining a distrust of her right to this distinction, I am glad to find myself in accord with Prof. Weber; since, however, the grounds upon which I base my distrust are not in all respects the same with his, I will first state and examine those which he brings forward.

A principal reason why Weber disbelieves the originality of the system of *nakshatras* is that no traces of it are to be found in India in the Vedic period proper, the period of the early hymns of the Rig-Veda. He would not, of course, urge this as a conclusive argument, since he would acknowledge that there was time enough between the period of the Vedic hymns and that of the Brâhmaṇas for such an institution to grow up, and darkness enough resting upon the interval to admit of its growing unobserved, until the system makes its appearance, complete and fully developed. And these considerations seem to me so greatly to limit the force of the argument as to forbid that any stress whatever be laid upon it: I should not feel in any appreciable degree less willing to admit the independent Indian origin of the *nakshatras*, on account of their non-appearance until the second period of recorded Indian history. There are passages in the later portions of the Vedic hymn-texts which furnish hints enough of a contemplation of relations between the moon and the *nakshatras* (that word being not yet used in its later and technical sense) on which to build the later development of the system, should other evidence favor such a construction of its history.

Weber adds three circumstances which appear to him to furnish ground for believing that Western Asia actually possessed a system of lunar asterisms or mansions of its own, which it must then have communicated to India. In the first place, a Mohammedan work, the Fihrist of Ennedim, of the tenth century after Christ, informs us that the Harranites had a custom of visiting their temples, sacrificing to the moon, eating and drinking, and performing other such ceremonies, on the twenty-seventh or twenty-eighth day of the month; of fasting twenty-seven days in honor of the lunar month; and so on. This, we are to believe, can only mean that the Harranites intended to celebrate the day on which the moon accomplished her sidereal revolution; and, since she revolves by moving forward so many degrees a day, from star to star, they must have recognized a series of lunar stations; and hence, they must have possessed the system of lunar asterisms! This argument is beyond all question the weakest point in Weber's whole essay, and of somewhat the same character with M. Biot's conversion of the Chinese encyclopedist's innocent parallel of the Chinese and Hindu asterisms into positive documentary proof of the derivation of the latter from the former. The festivals of the Harranites were very probably connected with the moon's periodical revolution, but how this implies their acceptance of a series of star-groups, selected to mark her daily progress along the ecliptic, it is impossible to see. If every nation which has observed the very obvious and elementary fact that the moon makes the round of the heavens in twenty-seven to twenty-eight days must be declared a partner in the system of lunar asterisms, how is our modern European astronomical science to escape? Why not with the same reason assume that every nation which made the year consist of twelve months had the signs of the zodiac, Aries, Taurus, Gemini, and the rest? And finally, supposing the Harranites even to be proved in possession of the asterismal system a number of centuries after Christ, why, on Weber's ground, should it not have come from the same source to them as to the nations about them? If the Hindu *nakshatras* could penetrate Asia from the Chinese Sea to the Arabian Gulf, as Weber supposes them to have done, they certainly might also reach the land of Harran.

The second ground on which Weber holds to the ultimate origin of the asterismal system in Western Asia, is that upon which he mainly based his earlier rejection of Biot's views: namely, that the word *mazzaloth* or *mazzaroth*, the apparent Hebrew equivalent of the Arabic *manzil*, occurs twice in the Bible, in Job (38. 32) and in 2 Kings (23. 5). This is more to the point, and may be a genuine and valuable evidence of the fact as claimed, but it is also clearly too faint and dubious to be relied upon with any confidence. The connection of the words *mazzaloth* and *manzil*, and the identity of the ideas which they represent, rather needs itself to be established by evidence derived from other quarters, than is capable of being employed to support a doubtful and controverted thesis.

Weber's third ground is the fact that the *manâzil* are spoken of in the Koran; in a general way, indeed, yet so as to show that they were well known and acknowledged, and held in high esteem. But, if we adopt Weber's views in other respects, this also appears to be of little or no account. He acknowledges the existence of the complete system of *sieu* in China as early as 250 B. C., yet thinks it only an offshoot of the Hindu *naksha tra* series. I ask again, then: if the Hindu system was so peri patetic in its character as to have made its way at that early date to remote China, why may it not have reached Arabia also in season to win there all the currency and authority which the Koran implies it to possess?

Finally, in the second part of his essay, Weber refers to the measure of the respective length of the day and night, when the sun is at either solstice, as laid down in the Jyotisha, and points out its inapplicability to any portion of India excepting its extreme north-western corner, while at the same time he indicates its near agreement with the actual fact at Babylon. In an additional note at the end of his whole work, he shows that the same measure, as nearly as possible, is given also by the Chinese writers. From this striking coincidence he draws, with considerable confidence, the conclusion that the datum must be one which has passed from Babylon into the possession of the other two peoples: and the farther inference would naturally be that both India and China must have received other astronomical truths and methods from the same quarter—among them, very possibly, the system of lunar asterisms. But there are serious drawbacks to the cogency of this conclusion and its corollaries. The latitude of Babylon and of that part of China in which were situated its ancient capitals and centres of civilization is so nearly the same, that independent observation of the respective length of the longest and shortest day would necessarily lead to nearly the same result in both countries. And the form of statement of the Chinese authorities is so vague and general as fully to cover any minor variation which we might otherwise have looked for. They say, in effect, that in midsummer the day is three fifths and the night two fifths, and that in midwinter the night is three fifths and the day two fifths, of the whole twenty-four hours, the nycthemeron.* That the Chinese had not the means and the skill, at a very early period in their history, to measure with at least this degree of accuracy the relative length of the day and night, could hardly be affirmed with any plausibility: and I should not therefore regard as a fact of any significance the correspondence of the Chinese reckoning with that which is inferrible for Babylon. Nor does such an item of knowledge appear likely to pass from the hands of one nation to those of another, considering the evident variability, and dependence on local circumstances, of the length of the day. There is more reason to suspect a derivation from some foreign source in the case of the Hindu datum, since Central India lies so far to the southward of Babylon that, even as the result of a rough determination, stated without attempt at precision, we should look for a considerable difference in the numbers given. Thus, at about the parallel of twenty-five degrees, or nearly in the latitude of Benares and the great cities on and near the Ganges, we should expect the greatest variation of the day to be stated at four *muhûrtas*, instead of six, or at less than a seventh of the whole day and night, instead of a fifth, as the Jyotisha gives it. Whether the assumption of a somewhat more northern place of observation within the limits of India proper, and a consideration of the inaccuracy of Hindu instruments and observers, would or would not sufficiently account for the character of the datum, allowing us to regard it as of native origin, I would not attempt to decide with authority: but I should not dare to rely upon it as any valuable evidence of astronomical communication from Babylon. Were the Hindu and Chinese measurements given with considerably greater exactness, down to minute parts of a *muhûrta* and a *khe*, and did they then agree closely with one another and with what the latitude of Babylon demanded, there would be ground for a probable argument; but such coarse data as are actually furnished us are next to worthless for use in the discussion of difficult and controverted points.

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* Their form of statement is, more precisely, that at the winter solstice the day is forty *khe* and the night sixty *khe*, and at the summer solstice the reverse; a *khe* being the hundredth part of a nycthemeron: but this really gives no more precision to the definition than I have described it above as possessing; or, at any rate, the utmost that could be claimed in behalf of it would be that it was made by tenths instead of fifths. That Biot should take the trouble to calculate the precise latitude, down even to seconds, and with allowance for the variation in the obliquity of the ecliptic, for the place where so rough an observation was made, is an actual marvel of uncritical and useless exactness. One might as well endeavor to fix the precise day and hour on which an author recorded his declaration that a certain eclipse happened "a hundred years ago," by rigorously determining, in the most scientific manner, the exact moment of the eclipse itself.

In Prof. Weber's arguments intended to prove or to render plausible the derivation of the *nakshatras* from Western Asia, we are thus able to find but little of which we can acknowledge the cogency. The form in which he holds the theory of the non-originality of the Hindu system is one that will commend itself, I am confident, to the acceptance of but few other scholars. He is not afraid to combine with it the belief that all the other known forms of the asterismal system derive themselves from the Hindu alone. But, if this be admitted, we lose nearly all inducement to inquire after any other and more ancient lunar zodiac, and shall be apt to call for very distinct and unequivocal evidence to convince us that any such ever existed. If there was a primeval system in the central position of Chaldea, why should the countries which lie all about that centre have received it by the circuitous route which leads through India? Or, if the Arabs once possessed it by direct derivation from its original owners, why should they have relinquished it later, to adopt the new form into which the Hindus had developed it?

The assumption of such an obsequious temper on the part of the Arabs, that, in deference to their Indian teachers, they should discard an asterismal series which they had long employed, and which the Koran had mentioned and ratified, is to me quite incredible; rather than admit it, I should be entirely ready to give up either the original derivation from Chaldea, or the later from India. That the Indian *nakshatras* were really adopted by the Arabs in place of their own asterisms, Weber regards as proved by the known communication of astronomical knowledge from India to Arabia in the early centuries of Islam, and the powerful influence exercised at that period by the Hindu science upon the Arab. But he overlooks one or two difficulties, which appear to me to vitiate his argument, and well-nigh overthrow his whole theory. We have, in the astronomical text-books, distinct and unequivocal information as to what the *nakshatras* were at a period certainly prior to this supposed communication. Whatever may have been the earlier changes of the system, the Siddhântas are entirely accordant as to what it had become in their day; and it is at least altogether probable that for some time previous there had been no important alterations made in it, or more traces of them would have come down to us. Is it to be credited, then, that when the Arabs were thus submissively giving up their own system in favor of the Hindu, they would at the same time have been independent enough to vary from their teachers in ten out of the twenty-eight groups, or more than one third of the whole number?* and yet more, that out of these ten they would have taken no less than six* from among those which, according to Weber, the Hindus had communicated to the Chinese nearly a thousand years earlier, but had themselves since given up? These discordances and coincidences appear to me to prove beyond all reasonable doubt that there has been no copying of the Hindu system on the part of the Arabs in the historical period; if they derived anything from India, it must have been at a time nearer to the remote date when the communication to China took place, if any such there was. Whether the Arabs were led more by Greek or by Hindu influence to change the beginning of their series from the correspondent of Kṛttikâ (Thuraiyâ) to that of Açvinî (Sharaṭân) is a very indifferent matter, and not worth debating, as it can have no bearing upon the question at issue. The change was no arbitrary one, but a natural consequence of the fact that the equinox had receded to near the beginning of Açvinî; the Greek and the Hindu astronomies would both equally have pointed out this fact and suggested the transfer; and the precept or example of either might have been followed, without any reference to the derivation of the *manâzil* themselves.

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* See the notes to the Sûrya-Siddhânta, chapter viii., especially the table on page 200 (Jour. Am. Or. Soc'y, vi. 344); also the comparative table given above (p. 45).

Whether, farther, the lunar zodiac of which the record is found in the Bundehesh is to be regarded as derived from a Hindu or an Arab source, or whether it is the direct descendant of a presumable original system different from either of these, is, of course, an open question, and to be decided, if at all, along with the general question of the origin and propagation of the asterismal system. If the sum of all the evidence gathered shall make it probable that the institution was first devised upon the banks of the Mesopotamian rivers, and spread thence to the surrounding countries, then it will seem most likely that Persia also received it from that quarter: if, on the other hand, India shall appear to be its true home, we shall have no difficulty in admitting that the series taught in the Pârsî scripture had no other source than India. The lateness and scantiness of our information concerning it puts it necessarily in this doubtful and subordinate position. But I cannot without a protest allow Weber to set it down summarily as of Hindu origin, upon the sole ground that the series as recorded appears to begin with the member corresponding to Açvinî. Besides the entirely dubious bearing of this fact in any connection, as set forth just above, there is here a special reason why the enumeration of the lunar asterisms could not begin otherwise than it does, even had the correspondent of Kṛttikâ been, to the apprehension of the Pârsîs, the one best entitled to stand at the head of the series. The document commences with stating the division of the zodiac into twelve signs, Aries, Taurus, etc.; these, it goes on to say, are divided, from their beginning, into twenty-eight portions, whose names are next rehearsed. How, in such a condition of the record, could the twenty-eight-fold division be made to begin otherwise than in accordance with the twelve-fold —that is to say, with Açvinî, the head of Aries?

What are then, finally, the true grounds for suspecting that India received her system of *nakshatras* from abroad? By far the strongest of them, in my opinion, is the appearance of the same system in the possession of so many other Asiatic nations, and in the case of the Chinese, at least, from so early a period. That the hypothesis of origination in India is positively excluded by these facts I would by no means claim: I only assert that they are much more readily explainable by supposing that the institution was first invented and applied at a central point like Chaldea, the seat of empire, commerce, and culture which are known to have had wide-reaching connections and influence in every direction. Hindu propagandism, so far as we know, began with Buddhism; and that Buddhist missionaries could have made the *nakshatras* a familiar and accepted institution in China as early as 250 B. C. is hardly to be believed. The claim advanced by Weber, that the development of the Tao philosophy shows such signs of Hindu influence as to make probable an ante-Buddhistic intercourse and communication between the two countries, is much too indefinite in form and doubtful in substance to be allowed any weight. To found important historical conclusions upon analogies between the doctrines of two schemes of mystical philosophy is, I should think, a proceeding of more than questionable soundness.

The force of this general argument of probability is supported by certain considerations derivable from the form of the Hindu system and the character of the Hindu people. As regards the former, the Arab and Chinese series seem to be at several points in nearer agreement with the presumable original order of the asterisms than is the Hindu; while the latter, which we can follow back in contemporary documents almost to the time of its first employment in India, shows no signs of change at these points, but rather the contrary. I find it hard to believe that Abhijit, Çravaṇa, and Çravishṭhâ (Lyra, Aquila, and Delphin) were included in the series as at first constituted; yet their presence among the *nakshatras* appears to me to go back beyond any time at which we could conceive the system communicable to other peoples—even to the very first period of the currency of the system in India. Again, the Hindus were not a people of such habits of mind that we should expect to see arise among them an institution like the lunar zodiac, of so practical a bearing, founded upon faithful and long-continued observations of the heavenly bodies, and intended for chronometrical uses. In the Hindus as students of the heavens, as observers of celestial conditions and phenomena for other than superstitious ends, my faith, I must acknowledge, is but the smallest. Their failure to notice the lesser planets until so late a time confirms me in this view. Throughout the whole period during which the system of lunar asterisms was in its full life and vigor, there is no mention to be found in Hindu authorities of any such bodies as planets: they do not make their appearance, as Weber has pointed out, until we meet with them in works showing the influence of the Greek astronomy. But it is not easily to be credited that a people who had so industriously and fruitfully contemplated the movement of the moon among the stellar bodies as to make an original and independent choice of twenty-eight groups of stars along her track for the purpose of marking her daily progress, could have failed to be struck by, and to make some account of, those other brilliant orbs which, like her, went round and round upon almost the same track. It is a vastly less difficult supposition that they may have borrowed the series from some other people, and have applied it to the only practical uses for which they felt its need—even giving it, in connection with those uses, a fuller development and greater prominence than it elsewhere received—without taking any particular notice of the other planets. Once more, the acuteness and good sense which could give origin to the institution as at first established are hardly reconcilable with the perversity which should lead to the substitution in India, in place of the primitive asterisms, of several groups lying so far away from the moon's track. I am unwilling to believe that the same nation which originated the system could at a later period compel it to endure such a disfiguration.

This is the array of probabilities upon which I base my suspicion that the Hindus did not, after all, originate the primitive system of lunar asterisms represented to us by the *nakshatras*, the *manâzil*, and the *sieu*. That it is but scanty, I freely admit: it is not of a character to compel belief, and I can quarrel with neither the candor nor the good sense of any one who shall refuse to be moved by it. I only claim that it is sufficient to prevent us from maintaining with confidence and dogmatism the derivation from India, either directly or indirectly, of the *manâzil* and the *sieu*, and to lead us to look with expectation rather than with incredulity for the appearance of evidence which shall show some central or western Asiatic race to have been the inventors of the lunar zodiac.*

I would add here a few remarks and explanations respecting one or two matters in Weber's essays not directly bearing upon the special point which we have hitherto been discussing.

Prof. Weber is still sanguine in his hope of making the Hindu names of the months—which are, by universal acknowledgment, derived from the *nakshatras* in which, during the period of a

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* Prof. Weber has kindly called my attention to the fact that M. Am. Sédillot, in his Matériaux pour servir à l'Histoire Comparée des Sciences Mathématiques chez les Grecs et les Orientaux (Paris, 1849; pp. 467–549), rejects Biot's views of the Chinese origin of the lunar zodiac, and claims the honor of its invention for the Arabs. As I have not access to his work, I cannot judge the grounds upon which he founds this claim; but neither can I believe them to be of a convincing character. It must be very difficult, even if the institution be actually of Arab origin, to prove the fact, because the Arabic literature is so much more modern than that of the Hindus and Chinese.

It may be worth while farther to note here that Lassen, in his very last work (Indische Alterthumskunde, Anhang zum iii. und iv. Bande; Leipzig, 1862; p. 72), pronounces Colebrooke to have shown (*dargethan*) that the Arabs received their lunar mansions from the Hindus. The statement is wholly and reprehensibly inaccurate: Colebrooke has neither done nor attempted to do any such thing. In the article to which Lassen refers (Essays, ii. 322), after expressing his inclination to the opinion, contrary to that of Sir William Jones, that the *manâzil* and *nakshatras* have a common origin, he says: "I apprehend that it must have been the Arabs who adopted (with slight variations) a division of the zodiac familiar to the Hindus. This, at least, seems to be more probable than the supposition, that the Indians reyear, the moon was full in each of her successive revolutions—yield a date as that of their fixation. To the remark made by us in the notes to the Sûrya-Siddhânta (xiv. 15, 16), that, in the nature of things, the moon's opposition to the sun must occur at all points of the zodiac in succession, and that hence no system of names derived from the *nakshatras* containing full moon could be continuously correct, he interposes the objection that we do not seem to take sufficient account of the *yuga*-period, of five or six years, within which the differences of solar and lunar time were reconciled. But he can hardly, I think, have made clear to himself what was the bearing and pertinency of this objection: to me it seems entirely irrelevant. The moon's opposition is a real phenomenon, depending solely on the times of revolution, and the varying rates of motion, of the sun and moon, and no artificial scheme of reconciliation of solar and lunar reckoning can possibly prevent its happening where those times and rates require, and so at all possible points on the ecliptic. In order to illustrate, however, by an example the relations of this question, I have calculated from the Nautical Almanac the longitude of the moon at her full in each month of a period of eleven years, from the beginning of 1854 to the end of 1864, and, by allowing in every case for precession, have reduced these longitudes to positions in the Hindu fixed sphere, determining the *nakshatra* in which each full moon occurred: reckoning, according to the Sûrya-Siddhânta, from a point ten minutes east of the star ζ Piscium as the commencement of division, and allowing thirteen degrees and twenty minutes of longitude to each asterism. During these eleven years, I find that full moon occurred six times in each of the asterisms Açvinî, Pûrva-Phalgunî, Citrâ, Anurâdhâ, Mûla, Uttara-Ashâḍhâ, and Uttara-Bhâdrapadâ; four times each in Kṛttikâ, Maghâ, Hasta, Viçâkhâ, Purva-Ashâḍhâ, and Revatî; and five times each in all the other fourteen asterisms. The same series of asterisms as places of

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ceived their system from the Arabians." . . . This was written in 1807. Ten years later, Colebrooke had come to hold the same opinion with much greater confidence; he says (Essays, ii. 447): "They [the Hindus] had a division of the ecliptic . . . . seemingly their own: it was certainly borrowed by the Arabians;" but he does not appear to have been strengthened in it by any new and deeper investigation of the subject, for he simply refers his readers, for authority, back to the other passage, quoted above. Lassen should have said, then, that Colebrooke conceived a suspicion, which time deepened into a persuasion, that the Arab lunar mansions were a copy of those of the Hindus: and nothing more. Lassen's looseness of statement upon this point is, I regret to say, much of a piece with his whole treatment of the subject of the Hindu astronomy. He seems not to have looked far enough into it to discover its difficulties, and his account by no means represents the results of the latest investigations of other scholars. To maintain, for instance, that Âryabhaṭṭa was the founder and author of the Hindu astronomical system, is very much like maintaining that Vyâsa was the author of the Vedas, epics, and Purâṇas: the former assertion, no less than the latter, suits the Indian archæology of fifty years ago far better than that of the present day.

the moon's full does not anywhere recur in two successive years: nor, indeed, is it possible that such should ever be the case; the names Çrâvaṇa, Bhâdrapada, Âçvina, and Kârttika can never be strictly applicable in four successive months, since the end of Çravaṇa is so far (92° 10′) from the beginning of Kṛttikâ, that the moon cannot possibly be full in the latter asterism the third time after she has been full in the former. But the series which commences in January 1854, and ends in September 1855, is precisely the same with that which begins in January 1862, and ends in September 1863.* Moreover, the Metonic cycle of nineteen years, being approximately a common multiple of the times of revolution of the sun and moon, and also nearly equal to twice the period of revolution of the moon's line of apsides, cannot fail usually to bring around the same series of asterisms, as stations of the full moon; by way of example, I have calculated the year 1844, and find it to correspond in every point with 1863. There is no year, among those included in this examination, during which the accepted names of the months would be derivable throughout from the asterisms in which the moon was actually full; but, twice in the period, there is a series of thirteen full-moon stations, of which eleven would give the true names of the months;† and, in the other two months, opposition between the sun and moon takes place at points removed, in the first series, only one and a quarter and one and two thirds degrees from the limits of the asterism which would give the true name, and, in the second series, only one half degree and four degrees, respectively. Upon either of these two successions of full-moon stations, now, as it seems to me, or upon any other of the same character, the actual scheme of month names might have been founded. We must beware of transferring to the period of origination of these names the precise divisions and exact observations of modern times. To lay out with accuracy the limits of the twenty-seven portions into which the ecliptic was divided was certainly not in the power of the ancient Hindu astronomers. Nor were they capable of fixing the precise place and moment of the moon's full. To their eyes she might seem to rise full, although she had already passed by a few degrees the exact point of opposition, or although she had not yet quite reached that point. Their skill is not to be supposed equal to the task of ascertaining just how far past the moment of full moon was, when it had occurred before sunset, or just how long it would be in coming, when it had not yet occurred at sunrise.

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* It is as follows: Jan., Pushya; Feb., Maghâ; Mar., Uttara-Phalgunî; April, Citrâ; May, Anurâdhâ; June, Mûla; July, Uttara-Ashâḍhâ; Aug., Çravishṭhâ; Sept., Pûrva-Bhâdrapadâ; Oct., Revatî; Nov., Bharaṇî; Dec., Mṛgaçiras; Jan., Punarvasu; Feb., Âçleshâ; Mar., Pûrva-Phalgunî; April, Citrâ; May, Viçâkhâ; May, again (June, 1863), Jyeshṭhâ; June (June again, 1863), Pûrva-Ashâḍhâ; July, Çravaṇa; Aug., Çatabhishaj; Sept., Uttara-Bhâdrapadâ. In Oct. 1855, the asterism is Açviṇî; in Oct. 1863, it is Bharaṇî.

† They are Sept. 1856 to Sept. 1857, and Sept. 1859 to Aug. 1860. The former of the two runs as follows: Sept. 1856, Uttara-Bhâdrapadâ; Oct., Açvinî; Nov., Kṛttikâ; Dec., Mṛgaçîrsha; Jan. 1857, Punarvasu; Feb., Maghâ; Mar., Uttara-Phalgunî; April, Çitrâ; May, Viçâkhâ; June, Jyeshṭhâ; July, Uttara-Ashâḍhâ; Aug., Çravishṭhâ; Sept., Pûrva-Bhâdrapadâ. In the other series, we have in Sept. 1859, Pûrva-Bhâdrapadâ; in Feb. 1860, Âçleshâ; July, Pûrva-Ashâḍhâ; August, Çravaṇa.

Considering, then, these two capital difficulties: first, the liability of any given series of full-moon stations to recur indefinitely, at longer or shorter intervals; and second, the inapplicability of calculations founded on an exact division of the ecliptic, and a precise determination of the place and moment of the moon's opposition, to the conditions of the primitive time when the months were named from the asterisms, it must be conceded, I think, that to hope for the ascertainment of that time by astronomical calculation is entirely futile, and that the calculator who, in compliance with Prof. Weber's invitation, should set himself at work to determine it, would show a vast deal more of enterprise than of good sense.

Another point with regard to which Prof. Weber appears to have fallen into error, is the relation to one another of Câitra and Phâlguna as spring months. More than once, in the course of his essays, he supposes himself to discover evidence that Câitra, as the first month of spring, has usurped the place earlier occupied by Phâlguna; and he finds the substitution a natural one, and accounted for by the precession. In this, however, he has precisely inverted the true relations, for the tendency of the precession is, in fact, to bring Phâlguna into that relation to the spring which was formerly held by Câitra. Thirteen centuries ago, the vernal equinox was at the beginning of Açvinî; at that time, therefore, when the moon was full in Citrâ, the opposite asterism, the sun was very nearly in the equinox. At present, the equinox has receded about eighteen degrees, into Bhâdrapadâs; hence, when the moon is full in Citrâ, the sun has considerably passed the equinoctial point, and the month Câitra falls at a later time in the spring than formerly, while the month Phâlguna, with the full moon in Uttara-Phalgunî, must much more often include the equinox. That is to say: Câitra, as a month occupying a given relation to spring, is nearly two thousand years earlier than Phâlguna, and nearly two thousand years later than Vâiçâkha, in the same relation. And if we find in the early Hindu authorities signs of a balancing between Câitra and Phâlguna as first month of the vernal season, it is only owing to that uncertainty and freedom of choice which obtains as to the determination of the beginning of spring; in part, also, as we may well suppose, to local variations of climate, which bring on the reawakening of Nature earlier in one region than in another.

This misapprehension, although in itself insignificant, has seemed to me to call for a few words of correction, in order to guard against an erroneous estimate of the chronological order of authorities favoring the one or the other month as the beginning of spring.

Other points of interest, suggested or discussed in these learned and fertile essays, must be left untouched here, as this paper has already extended itself much beyond its anticipated limits.

I cannot, however, quit the subject of the origin of the *nakshatras* without taking notice of the recently published views respecting it of Prof. MAX MÜLLER, of Oxford.* This scholar, in the Preface to the fourth volume of his valuable edition of the Rig-Veda and its commentary (London, 1862), has devoted considerable space (pp. xxxviii–lxxi) to a discussion of the question "Are the Indian Nakshatras of native or of foreign origin?" The date of the Preface is October 1862, and its publication appears to have taken place in the course of the next month; but its transmission to America was by some means so delayed that it did not reach my hands until April 1863, at which time the revisal of the foregoing article, and its preparation for the press, were so far advanced that I could not include the doctrines of Müller, along with those of Biot and Weber, in the same discussion. Since, however, so eminent an authority may not be passed by unheeded in an essay like the present, which is designed to be a summary and criticism of opinions respecting the relation of the Chinese and Hindu asterisms, I propose to go on and examine here, as succinctly as I can, his view and the arguments which he adduces in its support.

Prof. Müller declares himself unequivocally a believer in the originality of the *nakshatras.* He had "felt," as he informs us, on first reading, in 1846, the articles of Biot, "that the Brahmans could not have borrowed the idea of the Nakshatras from the Chinese." He wrote to the Asiatic Society of Bengal, in April 1860 (see Journ. As. Soc. Bengal, vol. xxix., 1860, p. 200), that he should never believe that the Hindus borrowed from the Chinese: he would as soon believe that the Greeks borrowed their astronomy from the Goths. If M. Biot had been cognizant of the sentiments thus expressed by so distinguished an Indianist, he would probably have penned with an increased intensity of feeling his denunciation of those philologists who decide important historical questions from an inward persuasion, rather than from an intelligent and dispassionate examination of the arguments brought forward on the one side and the other. And although he might have done injustice to Prof. Müller by ranking him in such company, I fear that his impression would have been strengthened rather than removed by the manner in which the latter opens his present discussion of the subject. On his first page, our author appears to set before us, not so much the inherent implausibility of Biot's views, and their discordance with well-established historical facts, as the great undesirableness of permitting ourselves to be persuaded by the arguments employed to support them. It is by an appeal to our prejudices, rather than to our reason, that he would nerve us to meet with incredulity and denial the confident claims of the great French astronomer. Vedic studies, he warns us, are likely to lose their chief charm for us, if we can be brought to believe that the Hindu mind was influenced at so early a period by foreign civilization. Moreover, if the Hindus should be supposed to have learned any astronomy from the Chinese, "would not the national individuality of the Aryan race be tainted in its core, and the Turanian man rise superior to his Aryan and Semitic brothers?" Now these are considerations which will have some value, and may even assume a controlling importance, with those who are more partizans of Vedic originality, and Indo-European superiority, than lovers of simple truth; but I should have thought it better and safer if Müller—who, as no one will doubt, is able to keep his own mind from being unduly swayed by them—had avoided the risk of warping the possibly weaker intellects of some of his readers, by laying before them such selfish inducements to judge the case unfairly. Moreover, I think that he considerably exaggerates the danger to which we should expose ourselves by too frank concessions to Chinese claims. The proper Vedic period is not liable to be robbed of its character of originality by proof that the system of lunar asterisms came from China, or from anywhere else, since the hymns of the Veda contain no trace of it, save a doubtful reference or two, in parts of the text presumably belonging to the second age. And as for the Brâhmaṇa period, we ought to rejoice if an opportunity were given us of throwing off a part of its responsibility upon other shoulders than those of the Hindus. No one has depicted its character more tellingly than Prof. Müller himself (see his History of Ancient Sanskrit Literature, p. 389): it is an era of superstition and puerility, which attracts less of our admiration and more of our contempt than any other in the long history of the Hindu mind. There is little hope, however, of our shifting its burden upon the Chinese, the Babylonians, or any other people out of India; for the borrowing of such an institution as the *nakshatras* does not necessarily imply any deep or abiding foreign influence. To claim, as Müller does, that the lunar zodiac is at the root of the Hindu sacred calendar, and that none of the sacrifices described in the Brâhmaṇas is conceivable without it, is, in my view, greatly to overrate its importance. The ancient calendar of the Hindus, as of other nations, undoubtedly consisted in the observation and reconciliation of the three natural and independent periods, the day, the synodical month, and the year, and did not need to rest on a system of asterisms. The libations of morning, noon, and evening, the ceremonies at new and full moon, the great sacrifices accompanying the notable operations of nature, as the recurrence of the harvests and the changes of the seasons, are not dependent on a lunar zodiac. This latter has only furnished a convenient nomenclature for the lunar periods, and a suitable frame-work for the observation and description of celestial phenomena. And such a handy mechanical device, as it may fairly be styled, is precisely of a character to pass from one people to another, as it appears to me, without involving the exertion of any potent and wide-reaching influence by the former upon the latter.

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* This addition to the original article was prepared in order to be presented to the Society at its annual meeting, in May 1863; but, being crowded out by the press of other matter, its communication was deferred until the next meeting at New Haven, in October 1863.

As regards the superiority of the Semitic and Indo-European varieties of humanity to the Chinese, I can hardly believe that Prof. Müller seriously regards it as hinging upon the question whether China did or did not invent the lunar zodiac. Our position as leaders in the world's history is too firmly founded to be so easily shaken. We do not need to feel smitten and humbled even when we have to confess that the Chinese were in possession of gunpowder, the mariner's compass, and the art of printing, long before we were, and perhaps furnished us one or all of these fundamental elements of our modern progress. We are secure enough in our place to be able to do the utmost justice, in great things as in small, to our poor neighbors of the furthest East. It cannot be denied that India might, with signal advantage, have learned much from China: had Chinese influence been powerful enough to engraft upon the wonderful acuteness and profound spirituality of the Hindu mind something more of the practical shrewdness, the concern for things sublunary, the patience of research, the respect for the arts of observation and record, which have always distinguished the inhabitants of the Middle Kingdom, it would have been a gift of inestimable value. Why, then, fear to acknowledge that the Hindus may have received from China an important element of their early astronomy, as they derived from Greece the whole groundwork of their later science? There was a time when the latter imputation also would have been scouted, as an insult to the antiquity and originality of Indian wisdom: if Indianists have learned to admit and reconcile themselves to it, they may much more easily become disposed to accept the former, if supported by sufficient proofs.

It may seem superfluous to protest against these expressions of Prof. Müller's in an essay a considerable part of which has been devoted to refuting M. Biot's arguments in favor of the Chinese origin of the lunar zodiac. But, while I regard all probabilities as opposed to the Chinese pretensions, I have yet not tried to prove them impossible, and I would fain see the way kept fairly open for demonstrating a claim on behalf of the Chinese, or of any other nation which shall finally be found entitled to the honor. The admission, into such an investigation, of questions respecting the charm of this or that literature, or the honor of this or that race, seems to me a thing to be deprecated. The wish to know the truth ought to be a sufficient motive to us in the prosecution of the inquiry: in history, a very little truth is worth more than a great deal of sentiment.

The grounds which Müller brings forward in support of his persuasion that the *nakshatras* cannot and must not be of any other than Hindu origin are, in my view, altogether untenable. He does not, any more than we formerly did in the notes to the Sûrya-Siddhânta, attempt a refutation of Biot's arguments for the originality of the *sieu*, but accepts them and their results *in toto*, even treating as an established and indisputable fact the most arbitrary and implausible of all Biot's assumptions: namely, that four of the *sieu* were added by Cheu-kong to an earlier system of twenty-four. He expresses, indeed, a general distrust of the authenticity of ancient Chinese dates and data, and even of the accuracy of identification of the stars limiting the *sieu*, but without entering into any detailed examination and criticism, like that which Weber has made so damaging to Biot's alleged authorities. But, while accepting Biot's premises, he rejects his conclusion: from the originality of the *sieu* he will not infer the derivation of the *nakshatras;* and for this he gives two reasons, which we will proceed to consider.

In the first place, he is not satisfied that any genetic bond of connection has been shown to exist between the Chinese and Hindu systems. He points out the fact, set forth in our table of the correspondences of the *sieu*, the *nakshatras*, and the *manâzil* (Journ. Am. Or. Soc'y, vi. 344; Sûrya-Siddhânta, p. 200), that the points of coincidence between the two former are only seventeen out of twenty-eight, or less than two thirds of the whole series, and thinks the agreement not so close as to be unequivocal evidence of identity of origin. But this is putting the subject of correspondences in a very imperfect light, by leaving out of the comparison the third member, the *manâzil*. The same table shows that the Chinese system agrees with either the Hindu or the Arab in twenty-three out of its twenty-eight constituent members, and each of these six additional correspondences is really as good evidence of a genetic connection between the *sieu* and *nakshatras*, as between the *sieu* and *manâzil.* If it be possible for one who compares only two of the three series to look upon their accordance as accidental, no one who views them all together can doubt that they are one and the same system. I need not dwell longer upon this objection, because the table given above (p. 45), with the accompanying explanations, has, as I hope, set the relationship of the three in a clear and convincing light, and also because Prof. Müller does not place his main reliance upon a denial of relationship, but candidly declares himself prepared to find that the discordances of the *sieu* and the *nakshatras* may be accounted for, and their community of origin demonstrated.

But, in the second place, after making this admission, Müller goes on to say (p. xlviii): "Every Sanskrit scholar will most readily surrender the whole system of the Târâs and Yoga-târâs, or junction-stars, whether twenty-seven or twenty-eight in number, as of foreign origin." Against this assertion I must enter my most earnest protest. If the asterisms, the groups of stars, are to be given up as of foreign—that is to say, of Chinese—origin, I can see nothing left to contend about: the *nakshatras* were borrowed from China. That Müller can hold to the independent Hindu origin of the system of asterisms, and yet believe the asterisms composing the system to have been obtained from abroad, seems almost a contradiction in terms: it is only to be explained and reconciled by taking into account Müller's peculiar view of the character of the system. In forming this view, he would seem to have followed the lead of Weber, adopting the latter's misapprehensions refuted in the earlier part of this essay, but with a modification which makes them still more objectionable. Weber, as we have seen, regards the system of "asterisms" (*nakshatra*) as primarily and chiefly a system of divisions of the ecliptic, only accompanied by a series of stars and star-groups, of which the particular members were indeterminate, and left to the selection of every system-maker. Müller goes yet further, and thinks it *merely* a system of theoretic divisions, having nothing to do with stars or groups of stars until a very modern date, when the example of the Chinese, or of some other nation, suggested to the Hindus the selection of an asterismal series, such as we find it in the astronomical textbooks. I cite the words in which this is laid down as a fundamental proposition, a kind of preliminary definition, the acceptance of which is necessary in order to the intelligent conduct of the discussion which is to follow: "Nakshatra has three principal meanings. Originally it meant stars in general; secondly, it meant the twenty-seven equal divisions of the heavens, constantly referred to in the Brâhmaṇas and Sûtras; and thirdly, it meant the twenty-eight asterisms, consisting of either one or more stars, which in later scientific treatises served as *points de départ* for astronomical observations. In order to avoid confusion I shall in future use Nakshatra in the second sense only, translating it by lunar mansions, while I use Târâ instead of Nakshatra, if employed in the third sense, translating it by asterism" (p. xlvii). Again he says (pp. xlviii–ix): "the whole system of the Târâs and Yoga-târâs .... never occurs in the ancient literature of the Brahmans, it is not mingled with any of their ancient rites or traditions. It comes in at a time when India was no longer shut out from the rest of the world, and when a scientific study of astronomy had succeeded to the homely rules of the Vedic priests." Once more, in the final summing up (pp. lxx–lxxi): "that Nakshatra, meaning originally stars in general, was fixed upon as the word to be used, κατ' ἐξοχήν, of the twenty-seven equal divisions of the starry sky, marked out by the periodical course of the moon; .... but that in later times certain stars, twenty-eight in number, and vaguely corresponding with the twenty-seven ancient Nakshatras, were selected for the purpose chiefly of fixing the movements of the planets, but likewise for other astronomical and astrological purposes."

It is unnecessary for me to express my opinion that this understanding of the character of the *nakshatras*, which totally divorces the star-groups from the regions of the sky marked and defined by them, is entirely erroneous. The preceding parts of this article have presented in more or less of detail the abundant evidence which shows that the *nakshatras* really are from the beginning what by their name they purport to be, asterisms, and not spaces in the heaven, or divisions of a circle; and I do not propose to repeat that evidence here, even in summary. Müller's view of their nature cannot, as I am persuaded, be sustained for a moment by any pertinent or solid argument. And it is to be noticed that he does not attempt to prove its correctness, or to show reason for believing that the Brâhmaṇas and Sûtras, when they speak of "asterisms," really mean "divisions:" he would have us grant him his whole thesis as a postulate. I will only proceed to point out some of the misapprehensions it implies, and the difficulties in which it involves its author.

His account of the meanings of *nakshatra,* I would first remark, is open to serious philological objection, as well as historical. That a word primarily signifying star in general should come next to mean a division of the heavens, and only in the third place a particular star or group of stars, would strike any one accustomed to trace out the genesis and connection of meanings as strange and hardly credible. The third signification, such a one would say, is clearly anterior to the second: the word meaning star in general must first have had its application restricted to certain stars or star-groups, and then have been used to designate the parts of the heavens in which these were situated: if it be not so, some special reason for the anomaly must be shown to exist. An additional indication that Müller's classification is erroneous is to be seen in the fact that it compels him to give up, as names for the *nakshatras* regarded in the light of stellar bodies, all the terms by which the Hindu authorities, the oldest and the newest alike, have been used to call them—namely, *nakshatra*, *bha*, *dhishṇya*, *ṛksha*, every one of them meaning 'star or constellation, asterism'—and to replace them by a new and strange word, *târâ*, 'star,' which, so far as I know, has never been employed in India to designate the lunar asterisms, and is quite unsuited to such a use, as it denotes a single star, while they were chiefly groups. The proposed nomenclature does not at all commend itself to our acceptance. If Müller's second and third meanings are to be distinguished, it should be by reserving *nakshatra* for the third, and borrowing from the astronomical treatises the term *bhoga*, 'portion' (or *bhabhoga*, *nakshatrabhoga*, 'portion of [the ecliptic belonging to] an asterism'), for the second.

Again, Prof. Müller altogether misunderstands our opinion, expressed in the notes to the Sûrya-Siddhânta, respecting the time of selection of the junction-stars of the asterisms. He more than once refers to us as having shown that it took place in the course of the sixth century after Christ. We, in fact (see Journ. Am. Or. Society, vi. 355–6; Sûrya-Siddhânta, pp. 211–2), only claimed that those positions of the junction-stars which are recorded in the astronomical text-books must have been determined at or near that period. That, for instance, the *dhruvaka* or 'permanent longitude' of β Delphini, the junction-star of Çravishṭhâ, was two hundred and ninety degrees, and its *vikshepa* or 'latitude' thirty-six degrees north, could only have been found by observation about the time referred to; but the choice of β to represent, in the calculation of conjunctions, the asterismal group composed of β, α, γ, and δ Delphini may have taken place as much earlier as we can see reason to suppose that the impulse to make it was felt. I do not, indeed, believe that it preceded by any considerable interval the actual definition of positions; but this is a mere matter of opinion, a corollary from my view of the history of the system, and it must be held carefully apart from a deduction legitimately drawn from astronomical data. Müller, however, commits the much more serious error of confounding together and treating as one the original establishment of the asterisms, and the selection of one star in each as junction-star. He is willing to surrender, as of foreign origin and recent date, "the whole system of the Târâs *and* Yoga-târâs, or junction-stars;" etc., etc. Now the selection of one star out of a group only implies the previous recognition of the group; it can show nothing respecting the date of that recognition. Even if we had proved that β Delphini was constituted junction-star of Çravishṭhâ in the sixth century of our era, the constellation Çravishṭhâ might have been a member of a system of asterisms five hundred, or a thousand, or ten thousand years earlier; not the faintest presumption of a definite date is capable of being carried from the selection of the junction-star to the establishment of the asterism whence it is taken.

Nor is our author more successful in his attempt to show how the Hindus should have been led by the example of the Chinese to graft upon their system of divisions of the heavens a series of star-groups, corresponding to a certain extent with the Chinese *sieu.* He points out that in the sixth century, the time of "selection" of the junction-stars, Chinese travellers were traversing India, and Hindu priests settled in China; and that a Hindu religion, with its accompanying train of Hindu festivals, presumably requiring regulation by a Hindu calendar, had been imported into the Middle Kingdom: and hence the necessity for a compromise between the astronomical grammars of the two nations. Now this would seem to be a good and sufficient reason why the Chinese should borrow the Hindu *nakshatras,* but I cannot see how it should account for the adoption by the Hindus of the Chinese *sieu.* If the Chinese could be led to import a religion from India, they might well enough take along with it an Indian calendar: but because emigrant Buddhist priests had taught a foreign nation to pray their prayers and keep their festivals, why should the Hindus at home, Brahmans, Buddhists, and all, have felt themselves under any obligation or any inducement to adopt an element of the astronomical system of that foreign nation which had no practical value for them, and of which, in anything like its proper form, they were unable to make use?

In connection with this part of his argument, Müller even refers to the addition of the twenty-eighth asterism, Abhijit, as made under Chinese influence, although, as we have seen above, that asterism is mentioned along with the others, in all respects as if one of them, in several authorities of the Brâhmaṇa and Sûtra periods, going back almost to the time of first appearance of the *nakshatra* system in India.

It must not fail, however, to be noted that in a single isolated passage Prof. Müller puts forth an opinion which would seem to show that he holds, after all, the true view of the character of the asterisms, and believes that the constellations bearing the names of the *nakshatras* were noticed and named by the Hindus before the division of the planetary path into lunar mansions was made. He says (pp. lxi-ii): "I hold then that the names given to stars or constellations, such as Kṛttikâ etc., existed before the triseinadic division of the heavens became fixed; that the most interesting, though not always the most brilliant stars were selected to serve as exponents of the twenty-seven divisions." How this is to be reconciled with what he has elsewhere taught us respecting the restriction of the term *nakshatra*, in the second period of its use, to divisions of the heavens, and the late selection, under foreign influence, of the *târâs*, or constellations, and their *yogatârâs*, or junction-stars, I do not at all see. The two doctrines, put together, would seem necessarily to lead to some such result as this: Before the specialization of the word *nakshatra*, a certain group of stars had attracted the attention of the Hindus, and they called it the *nakshatra* ('asterism' or 'constellation') Kṛttikâ. When, however, they next fell upon the idea of dividing the heavens into equal spaces of thirteen and a third degrees each, they gave to one space the name of the *nakshatra* ('division') Kṛttikâ, without reference to a star-group, or remembrance that any such had ever borne the same title. But finally, when they found the Chinese paying particular attention to one of the stars in the constellation of the Pleiades, which happened to be situated in that part of the sky which they had lately been calling the *nakshatra* ('division') Kṛttikâ, and also, still more remarkably, to coincide with the group which they had anciently styled the *nakshatra* ('asterism') Kṛttikâ, they "selected" the same constellation anew, and commenced calling it once more by its old name—and so of the others in the series. I do not in the least suppose that Müller consciously holds this doctrine, the simple statement of which is its sufficient refutation, but I am unable to combine in a different manner the separate items of his argument. There must be, as I cannot but believe, some indistinctness of view on his part, which prevents him from noticing the inconsistency of the separate opinions which he puts forth in different parts of his essay, or else some infelicity of expression, which forbids me rightly to apprehend what he intends to assert. Some of his statements are evidently ambiguous, admitting of more than one interpretation; but I have not succeeded in deriving from a diligent comparison of them any connected account of the history and character of the *nakshatras* which harmonizes them all, or which does not appear to be unequivocally at variance with some argument used, or some opinion recorded, in one or another part of the discussion.

We must, indeed—as Prof. Müller urges, in replying to our opinions advanced in the notes to the Sûrya-Siddhânta respecting the relation of Abhijit to the *nakshatra* system—distinguish carefully between the twenty-seven equal portions of the ecliptic and the twenty-seven groups of stars constituting the series of asterisms proper: that is to say, as I should interpret the caution, we must beware of confounding the *nakshatras*, which are constellations, with the regions of the sky defined and named by them, which are spaces or arcs. It would be an equally fatal omission, too, when we find a series of star-groups and a system of divisions so connected and identified as to be called by the same names, not to inquire carefully how they stand related to one another, and to which of the two the nomenclature originally and properly belongs. We are in no danger of mixing up in our minds the Greek signs of the zodiac with the *dodecatemoria*, or twelfths of the ecliptic; but neither do we view the two as unconnected, and suppose that the mere idea of a duodecimal division constitutes a zodiac. Müller's doctrine of the priority and independence of the divisions of the lunar zodiac seems equivalent to regarding Aries, Taurus, Gemini, etc., as names of ideal twelfth parts of the ecliptic, for which signs or constellations were at a later period invented. When we find zodiacal "signs" spoken of, and read of the Twins, the Scorpion, the Goat, and so on, we know that we are dealing with constellations, and with divisions only as founded on and determined by constellations: in like manner, when we meet with a system of "asterisms" (*nakshatra*), and find it composed of such members as the Ruddy (*rohiṇî*), the Hand (*hasta*), the Brilliant (*citrâ*), the two pairs of Beautiful Feet (*bhadrapadâs*), and the like, we cannot help drawing the same conclusion.

That the Greeks first divided the constellation of the Scorpion into two zodiacal signs, the Scorpion and the Claws, before the latter was set off and called the Balance, is in close analogy with the Hindu division of the three constellations Phalgunyas, Ashâḍhâs, and Bhadrapadâs, into two lunar asterisms each.

Whether Prof. Müller's understanding of the month and its relations is altogether correct, I am not quite sure; he has at least, I think, sometimes expressed himself with regard to it in a manner liable to misconstruction. "Lunar chronology," he tells us (p. lxix), "seems everywhere to have preceded solar chronology;" and he goes on to prove it by evidence derived from various quarters. But the assertion is almost as much a truism as would be the statement that terrestrial chronology is more primitive than lunar. No measure of time is so forced upon man's attention and recognition, so enters into and affects his whole physical and moral life, as the earth's revolution on her axis, which, slightly modified by combination with her revolution about the sun, produces the alternation of day and night. A human language with no word for "day" in it is an almost inconceivable anomaly. The next most conspicuous natural period is that of the waxing and waning of the earth's satellite, or the synodical month, and it is doubtful whether any people or tribe was ever met with, so careless of the operations and manifestations of Nature as never to have measured time by moons. In his extract from his own review of Mr. Hardwick, Prof. Müller says (p. xli) that "the primitive division of the year into lunar months must have taken place previously to the first separation of the Aryan family." I presume we have here merely a carelessness of expression, for Müller certainly would not seriously maintain that the month was a result of the division of the year into parts: as well might one hold that the day was a result of the subdivision of the month or week. The hour, indeed, is an artificially determined part of the day, and, when found in the possession of two peoples, is good evidence of a communication from one to the other, or to both from a common source; and the same is true of the week, as constituent part of the month; but day, month, and year are perfectly independent natural periods, which cannot but be observed alike by all nations. The year is brought to notice by meteorological changes, by the succession of the seasons, and it assumes different degrees of prominence and importance according to the latitude, the other natural conditions, or the mode of life, of different peoples: its conception as a period, the determination of its limits, the connection of its phenomena with the apparent movement of the sun, the assignment to it of a suitable name—all these are results of a closer, a longer continued, a more widely extended process of observation than is necessary in the case of the two lesser measures of time. In how many nations, even of considerable cultivation, do we find the process still incomplete, and time measured only by moons, or, in a larger way, by springs, by harvests, by rainy seasons, or the like.

The three natural periods which we have been considering are, of course, the solar day, the synodical month, and the tropical year. The sidereal day, the periodical month, the sidereal and anomalistic years, etc., have quite a different character, being, as it were, artificial measures of time, learned variations of the others, and indicative of a considerable advance in the art of observation of the heavenly bodies: their establishment and use is not to be inferred for any ancient people except from direct evidence. Even the possession of the lunar zodiac, the system of *nakshatras*, does not imply the practical employment of the periodical month as a chronological element, although it supposes a recognition of the period of the moon's sidereal revolution. One of the earliest and most important uses to which the

Hindus put the *nakshatras* was, as we have seen, to furnish a nomenclature for the series of synodical months. It has not been shown, so far as I am aware, that the month of twenty-seven days, or a little over, ever made any figure in the Hindu reckoning of time. That, in the artificial scheming in which the Hindus delight, they have set up months of this character, as well as of several others, and have formed years containing twelve, thirteen, and fourteen of them (years of 324, of 351, and of 378 days: see Weber's second essay, p. 281 etc.), is quite in order, but proves nothing as to the practical use and value of the period: we have nearly the same reason to suspect, when a Hindu author speaks of a year, that he means one of those unnatural and absurd combinations, as that, when he speaks of a month, he intends a periodical month. Liberal as the Sûrya-Siddhânta is in its enumeration of the different kinds of months, it knows of none which depends for its measure upon the moon's sidereal revolution.* On the one hand, therefore, I am of opinion that Weber over-estimates the importance in Hindu antiquity of this chronological element, to which he has directed attention in one or two passages of his essays; and, on the other hand, I fail to see the justification of Müller's stricture upon Weber, given in the following words (p. xlvi., note): "What vitiates many of Prof. Weber's arguments is that he has not made it clear to himself in every instance whether the months he is speaking about are sidereal or synodical." If Müller had only taken the trouble to point out an instance or two where the failure to make such a distinction had misled Weber, and rendered his conclusions nugatory, we should have better understood what value to attach to this criticism: as the case stands, I must confess that I have not noted any cases where it applies; and on this account, as well as for the general reasons set forth above, I cannot but suspect it to be founded only in a misapprehension on the part of Müller himself.†

In Müller's estimate of the value to be attached to the Jyotisha's account of the position of the colures as a chronological datum, I fully concur. That position certainly yields as its exact date (supposing that the limits of the asterismal portions were precisely the same when the series commenced with Kṛttikâ as when, later, it was reckoned from the beginning of Açvinî) the twelfth century before Christ, and not the fourteenth, as Colebrooke supposed; but there are for the present, at least, and there are likely always to remain, insuperable difficulties in the way of drawing from it any date employable in constructing the framework of Indian chronology: we cannot be certain, in the first place, that the observation is of Hindu making, and not imported from abroad; again, admitting that it was made in India, we should have to allow it a range of several centuries, on account of its own difficulty, and the inaccuracy of the Hindus as observers; and finally, we have no means of attaching it definitely to any work, or class of works, or period, in the Vedic literature.

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* Its *nâkshatra mâsa* (i. 12) is of quite another kind, being thirty sidereal days, or true revolutions of the earth on its axis.

† In the same note (on p. xlvi), Müller appears to me to come short of doing justice to Weber in one or two other particulars. When taking pains to say "I differ from nearly all the conclusions at which Prof. Weber arrives," he might better have added "as to the relations of the Chinese and Hindu asterisms;" for, as regards the Hindu system itself, Müller's opinions accord more nearly with Weber's than with those of any other authority who has treated the subject. Nor was he justified in referring to Biot as having treated Weber's essays with "withering contempt," even though he added at the same time his opinion that they "hardly deserved" it. The expression involves, in the first place, a gross error of fact, since the longer and more important of Weber's essays was not published until after

It remains to say something upon certain points brought forward and commended to our consideration by Prof. Müller, in the earlier part of his Preface, from Bentley's work on Hindu Astronomy. The former approves, upon the whole, the severe verdict of condemnation which, after as full and dispassionate a study of Bentley's lucubrations, I think, as any one has ever given them, we pronounced upon him and them in the notes to the Sûrya-Siddhânta. Yet he cannot resist the temptation to lay before us, in his Preface, long *verbatim* extracts from the Historical View of the Hindu Astronomy, neither ratifying nor rejecting them, but simply placing them before our eyes, and

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Biot's death, and so could never have been greeted by the latter with either approbation or contempt. And, in the second place, the epithet "withering" was certainly very unfortunately chosen. We do not use that epithet except when we intend to imply that, in our opinion, the subject of the contempt is withered, or ought to be withered, by it; and to attribute to it that power in the present instance would be palpably most unjust. Each of the two disputants, it may be said, contemned the other's method and arguments; Weber depreciating the scientific mode of research as compared with the philological, Biot setting at naught the philological reasonings and critical doubts of an adversary who confessed inability to meet him on scientific ground. If we may judge by results, Weber's contempt ought to be the more "withering" of the two; for while his essays will always remain a most valuable mine of documentary information respecting the *nakshatras*, it is certain that all Biot's labors upon the *sieu* and the *nakshatras*—whatever may be the value in other respects of his researches in the Chinese astronomy: of that I am no competent judge—have done nothing but embroil the subject; by casting his immense weight of authority on the side of a most untenable theory, he has greatly impeded the general adoption of the true view. Biot, as we have seen, did not deal gently with those Indianists who differed from his opinions upon this matter, and Müller should have been restrained by a fellow-feeling from too vividly depicting Weber as prostrate beneath his contempt, seeing that it would also have been poured out without stint upon one who, like himself, not only refused to believe that the *nakshatras* were a mere distorted imitation of the *sieu*, but even doubted whether the two systems were genetically connected.

leaving us to draw what conclusions we may or can from them: he himself thinking them "deserving of more attention than they have hitherto received." In this opinion I cannot agree with him: I believe that they have already met with all the attention due them, and, being readily accessible where they stand, were likely to get all that they should hereafter be found to merit; and it seems to me hardly consonant with the dignity of the editor of the Rig-Veda and author of the History of Ancient Sanskrit Literature thus to make himself the uncriticizing reporter of the wild speculations of a confessedly unsafe authority. We expect from a scholar of the position and reputation of Müller actual guidance through the dark places of Hindu archæology, and not a mere introduction to such blind leaders as Bentley. Since, however, he has chosen to lend a *quasi* sanction to the latter's reasonings and their results—for, let him qualify his approval of them, and caution us against their possible unsoundness, as much as he will, his deeming them worthy of quotation in such a place must give them a currency and consideration which they would never of themselves have attained—I cannot refrain from accepting his challenge, and submitting them to an examination which shall attempt to show what they are really worth.

Bentley claims that the Hindu names of the months, derived from the lunar asterisms, could not have been formed earlier than 1181 B. C. (not, as Müller inadvertently states, that they could have been formed only in 1181 B. C.). The month Çrâvaṇa, he says, *always began at the summer solstice,* and it was not until the date mentioned that the solstice came, by the precession, to stand opposite to the end of the asterism Çravaṇa. And as, by universal consent, the moon's opposition in Çravaṇa gave name to the month Çrâvaṇa, it follows, with the force of a mathematical demonstration, that the name cannot be older than 1181 B. C. Now this argument is beautifully characteristic of Bentley. Allow him to make his own premises, and he has astronomical knowledge enough to draw from them, in many cases, the true conclusion. It is very much his habit, too, to make his premises: his most arbitrary assumptions, his most daring conjectures, he puts forward with the same unflinching confidence which might attend the announcement of a truism, and they are therefore apt to impose upon those who have not learned what his authority is worth, and have been incautious enough to trust themselves to his guidance. The whole calculation, in this case, hinges on the alleged fact that the Hindus always began their month Çrâvaṇa at the summer solstice. It is asserted by Bentley as if it were an acknowledged and indisputable truth; but it is, on the contrary, as I believe, utterly false. Being a statement of a purely historical character, its credibility must repose, of course, on documentary evidence; and here is where our author's great lore, and rarely equalled familiarity with the ancient Hindu literature, might most worthily have been brought in to aid the astronomer Bentley, who knew little if any Sanskrit, and was wanting to a rare degree in philological and historical learning and judgment. If Müller had proved to us that the other was right in this one particular, he would have rendered the calculation and the resulting date truly valuable for the history of Hindu astronomy. But he has not done so, nor is it probable that any one will ever be able to do so. Bentley's assertion implies that the months to which the asterismal names originally belonged were solar months, twelfth parts of the tropical year. But, if we know anything about the matter from the literature of the period during which they came into use, we know that they were applied to true lunar months, synodical revolutions of the moon. Solar months, and equinoxes and solstices as division-marks in the year, are unknown to the Brâhmaṇas. Even in the modern system of the Hindus, and in the division of the months according to solar reckoning, Çrâvaṇa does not begin at the summer solstice: for the modern Hindu year is sidereal, and its months commence when the sun arrives at certain fixed points in the heavens, from which the equinoxes and solstices are becoming farther removed with the lapse of each successive year. The name Çrâvaṇa, then, was first given to that lunar month, or synodical revolution of the moon, during which she was full in the asterism Çravaṇa, and it might as well have been given five thousand years before Christ as twelve hundred. Bentley's calculation is not worth the paper upon which it was originally printed.

A second calculation and deduction, quoted by Prof. Müller from the same authority, possesses a somewhat greater apparent plausibility, and does not admit of being disposed of so summarily. The "ancient astronomers," Bentley discovers, assert that four of the planets were sons of four of the lunar asterisms, the spouses of the moon: namely, Mercury of Rohiṇî, Venus of Maghâ, Mars of Ashâḍhâ, and Jupiter of Pûrva-Phalgunî; and that the moon, their father, was present at the birth of each. With his usual courage and enterprise, he sets himself (or sets some one else: see farther on) to find a physical explanation of the legend, and to make it yield a date. He imagines, in the first place, that the birth of a planet from an asterism in the presence of the moon will most naturally be explained by an occultation of that planet by the moon in the given asterism. In the course of the last three or four thousand years, however, the moon must have occulted each of the planets a great many times, and in all or almost all the different asterisms; hence we shall be able to derive no date from this circumstance alone.

But the attribution of such names to only four of the planets affords ground for conjecturing that occultations of these four within some limited period may have furnished the occasion of the nomenclature. By retrospective calculation, a single year appears to be found, in the course of which the moon occulted these four planets, and these only, and each of them in the asterism which is reputed to be its mother. The year is 1425–1424 B. C. During the course of it, Saturn, the only other planet, was out of the moon's way, and could not be occulted. Bentley asserts, then, not doubtfully, but with his usual confidence, and as if the truth of the matter was beyond all question, that the observations of the occultations were made, and the names given, in the year 1425–4 B. C., and he establishes this as one of the principal dates in his history of ancient Hindu astronomy.

In examining this process, I wish first to point out that it is probably not Bentley's own, but given by him at second hand. The four dates are: August 19, 1425, Venus occulted in Maghâ; April 17, 1424, Mercury occulted in Rohiṇî; April 23, 1424, Jupiter occulted in Pûrva-Phalgunî; August 19, 1424, Mars occulted in Pûrva-Ashâḍhâ: the included time is precisely a year. But, in reporting these dates, Bentley commits the almost incredible blunder of reckoning them to cover a period of sixteen months, as if the time were after Christ instead of before, and so April 17, 1424 the earliest, and August 19, 1425 the latest of them! This he does not by inadvertence merely: he repeats the statement twice, in text and note, and arranges the four dates in a corresponding order.* To my mind, this is almost conclusive proof that he did not himself make the calculation, but got it from some one else, and bungled it in the reporting. For it is perfectly evident that the gist of the calculation as made, and the force of the conclusion drawn from it, lay in no small measure in the fact of its covering just a year. If an odd time were permitted to be taken, why not eighteen months, or two years and a half, or a longer period, during which occultations in the same asterisms might be found to have taken place? And it is hard to conceive that Bentley, if the work was his own, could have forgotten this important circumstance when he came to put it in shape for publication.

In the second place, I cannot but think the whole story, as reported by Bentley, of somewhat doubtful authenticity. Not that the four planets which it concerns do not bear in Hindu usage names indicative of their birth from the several asterisms mentioned: Müller, at the end of his Preface (p. lxxxviii), is able to quote authorities for them all. But whether there is on record a legend restricting the nomenclature to these four planets, and adding the distinctive circumstance that the moon was in each case present at their birth, seems more questionable. Bentley, as is his wont, gives no authority for it; Müller has evidently found none; nor had Wilson, who refers to the story and its explanation by Bentley in the notes to his Vishṇu Purâṇa (p. 225).* I have never seen any reason for believing Bentley a dishonest reporter, or capable of deliberately falsifying a citation for the purpose of making it support one of his theories; but the interposition of another between him and the original authority, which I have just shown to be with great probability assumable, notably facilitates the supposition of a tampering with the sources: we all know how easy it is for a story going through two hands—one, perhaps, a little unscrupulous, the other a little eager and credulous—to receive a dressing-up which somewhat changes its aspect. The circumstance which awakens suspicion is that, in fact, Saturn also, no less than the other planets, has a name implying his birth from a lunar asterism, namely Revatî. This is pointed out by Müller (p. lxxxviii†). It looks to me a little as if Bentley's friend had left Saturn out of the account because he found it an impracticable subject, and had added the trait respecting the moon's presence in order to give the more plausibility to his explanation of the names by the supposition of occultations. Still, I should not be inclined to push this suspicion too far, nor loth to be convinced of the existence of Bentley's legend in the precise form in which he reports it.

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* That Müller copies all from him without noticing the blunder is not, perhaps, to be wondered at.

Again, occultations by the moon are not an altogether unexceptionable explanation of a legendary birth of the planets. In an occultation, as in an eclipse, the obliteration of the planet, its disappearance, is a much more striking phenomenon than its reappearance. We should expect, then, that a legend founded on occultations would speak of a death rather than a birth; or, if of a birth, then at any rate of a new birth, a regeneration. If there was aught in the story which implied a destruction and re-creation, a swallowing and disgorging, or the like, Bentley's interpretation of it would possess a much more evident plausibility. Further, we have a right to ask whether there is anything so very rare and striking in the occurrence of four occultations during the course of a single year as should naturally lead to the founding upon it of a nomenclature of the planets.

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* Müller is clearly in error, I think, in stating that Wilson "speaks distinctly of the legend as occurring in the Vâyu and Linga Purâṇas." Wilson says nothing at all of the legend, but only of the positions of the planets at the end of the Câkshusha Manvantara, as given by those authorities.

† Where, in the seventeenth line, "Saturn" is to be read for "Jupiter," as the context also plainly shows.

The question here is not whether 1425–4 B. C. is the only single year when the moon can have occulted the four planets in the asterisms which give them their names; this might readily be granted to Bentley's claim, but would only serve to fix the year, supposing the interpretation of the legend to be indisputably correct: when we are testing the character and value of the interpretation itself, we must also inquire whether there may not have been other years when the moon has occulted four planets, or three, or five, in other parts of the heavens, and when, accordingly, a planetary nomenclature would have been equally likely to be suggested. The occultation of a brilliant planet by the moon is a beautiful phenomenon, likely to attract the attention of a people given to assiduous contemplation of the heavens, and accustomed to watch the planetary motions and aspects; a succession of four phenomena of the kind within a limited period, as a year, would be yet more striking to such a people; but if this may happen several times in the course of two or three thousand years—and, though unable to speak with authority upon the subject, I should certainly think it might—then no people could be pronounced likely to select one case from among the many, and to make it alone the basis of a planetary nomenclature. To render such a supposition plausible, we should at least require some independent evidence going to show that the names possessed about the antiquity which it would imply. But, far from this, we have the very best reason for believing that the Hindus had not taken any notice of the planets at all down to a time fifteen centuries, and more, later than that which Bentley's calculation would assign to the names here in question. This important fact, that the first mention of the lesser planets is in India contemporaneous with traces of Western astronomical science, has been more than once referred to in the preceding pages, and its bearing upon such speculations as these of Bentley's is of the most decisive and crushing kind. He who, in the face of it, would prove that the Hindus gave to the planets a certain series of appellations more than fourteen centuries before Christ, must do it by something more convincing than a doubtful interpretation of a modern Puranic legend.

It may be asked whether, if Bentley's explanation be set aside as unsatisfactory, any other can be found which shall be less liable to objection. I answer, yes: such an explanation has been derived by Prof. Müller himself from Wilson's notes to the Vishṇu Purâṇa. The fact has already been noticed that Saturn, like the other planets, has an asterismal name, being denominated "son of Revatî." For Mercury, two such metronymics are found in the Hindu lexica: he is styled "son of Çravishṭhâ," as well as "son of Rohiṇî." The moon's ascending and descending nodes have appellations of the same character:

they are called sons of Bharaṇî and of Âçleshâ, respectively. Of this class of designations, then, we have eight, six for the five planets, and two for the moon's nodes. Now no less than six of the eight, including two of those which Bentley professes to account for, are explained by the positions which the heavenly bodies bearing them are by certain of the Purâṇas declared to have occupied, at an epoch which they call the Great Equinox, and which the Vâyu Purâṇa states to be the end of the Patriarchate (*manvantara*) of the Manu Câkshusha (see Wilson's Vishṇu Purâṇa, p. 225, note). This Patriarchate directly preceded the present one, which is that of the Manu Vâivasvata, and began about a hundred and twenty million years ago (see Sûrya-Siddhânta, i. 18–23, and notes). The Hindu conception apparently was that the end of the former order of things and beginning of the present was an era so important in the history of our world that it constituted a regeneration; and that the planets might be regarded as born, for us, each from that region of the sky which it then occupied. That this explanation is far preferable to Bentley's, for the names to which it applies, on account both of its including a larger number of them and its greater inherent plausibility, is to me very obvious. And the probability is of the strongest that the two appellations which it does not cover will yet be satisfactorily accounted for in a similar manner.*

In view of all these difficulties and objections, I maintain, with much confidence, that Bentley's explanation must be pronounced a failure: the probability that four occultations, happening in the year 1425–4 B. C., suggested four of the Puranic names of the planets, is very small indeed, compared with the probability that those names had another origin, and that their correspondence with the moon's occultations in the year designated, if such correspondence there be, is only a curious accident.

I say, if such correspondence there be; for it must not fail to be observed that, even without testing Bentley's processes by a re-calculation, we may note sundry considerations which cast a strong shade of doubt over their validity. It is greatly to be questioned whether the astronomical science of Bentley's time knew the movements of the moon and lesser planets with sufficient accuracy to be able to determine with confidence, at a distance in the past of more than three thousand years, the fact of an occultation as visible in a particular country at a particular time—if, indeed, the astronomy of our own day is equal to so delicate an operation. Moreover, an occultation of Mercury, which has to be effected so near the sun, generally in the strong twilight, and by a moon of which scarce a rim is to be seen, must require such a combination of favorable circumstances to make it visible with the naked eye, that he who aserts the phenomenon to have been seen in India in the year 1424 B. C. should be called upon, under penalty of disbelief of his assertion, to show that its conditions were such as to render the sight possible without aid from the telescope. Bentley's calculations concern matters of such difficulty, and the results they yield are so baldly stated, that the accuracy of the former and the conclusive force of the latter are alike open to just suspicion. And this is all we need to fortify us in our conclusion, already pretty well established by other evidence, that his whole interpretation is too weak to sustain itself, and must be rejected as valueless.

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* There is something unaccountably strange in the Puranic definition of the positions of Mercury and Venus at this important epoch: how Mercury can be, even by a Hindu cosmogonist, placed in Çravishṭhâ when Venus in put in Pushya, at least 146° 40′ distant from him; or how either can receive such location when the sun is made to stand in Viçâkhâ, at least 93° 20′ from Venus, and at least 80° from Mercury, it is hard enough to see. The furthest distance from the sun actually attained by Venus is about 48°; by Mercury, 29°; so that they can never actually be more than 77° apart: nor are the greatest elongations of the two planets, as determined by the modern Hindu astronomy, very different from these. It looks as if the defined positions of the planets at the Great Equinox were mere guess-work, and the work, too, of a very unlearned and blundering guesser, rather than found by retrospective calculation. The putting of the moon's nodes, also, in Bharaṇî and Âçleshâ—or, at the utmost, only 106° 40′ apart—is a yet grosser error of the same character.

Thanks to Prof. Müller, however, we are not left only to this critical balancing of seemingly opposite probabilities. He has done us the service of moving competent astronomical authority to test the accuracy of Bentley's calculations, and with the most satisfactory results. Mr. Hind, of the British Nautical Almanac Office, has performed over again the whole process, with the aid of the best modern tables, and has found that "Jupiter is the only planet that could have been occulted." On or about the dates at which Bentley claims the other occultations to have taken place, the moon passed the three other planets in conjunction: Venus, at the distance of a little less than two degrees, which was a near approach; but Mercury and Mars, at a removal in latitude of not less than six and a half and seven degrees respectively. This, of course, utterly annihilates Bentley's chronological deduction. To give the latter even the faint shadow of possibility which at the best belongs to it, we must positively have occultations. The supposition that the Hindus of 1424 B. C. would have thought of deriving a nomenclature for the planets from the fact of the moon's approach to them, in the course of a single year, within distances varying from half a degree (with perhaps an occultation in this case*) up to seven degrees, is so devoid of plausibility that it does not merit an instant's consideration. Passing strange is it, however, that Müller himself does not see this. After presenting Mr. Hind's figures, he says (p. lxxxvii): "It will thus be seen that the statement of Bentley as to the dates of the four conjunctions of the moon and the planets is fully borne out by Mr. Hind's calculations, and that the coincidence between the legend quoted by Bentley and the astronomical facts determined by Mr. Hind is a real one." As if Bentley had said anything about "conjunctions;" and as if a conjectural explanation of a legend which absolutely requires occultations to support it, and cannot quite stand even then, could be bolstered up with conjunctions! Bentley himself knew too much of astronomy ever to have thought of maintaining such an absurdity. Suppose that we had a Puranic legend which admitted, with a good deal of doubt, of interpretation as founded upon a total eclipse of the sun in a certain asterism, visible in India; and that some venturesome interpreter had so explained it, and had, by calculating the eclipse, assigned it a certain date; and suppose that some much more competent astronomer, on revising the calculation, had found that the eclipse, after all, was not total, nor visible in India; that, in fact, there was no eclipse at all; but that, on the day claimed, the moon did actually pass the sun, at the given point in the heavens, at the distance of two or three degrees from it: should we think this a confirmation of the first interpreter's work? Even Prof. Müller, I presume, would not so regard it; and yet, the case is closely analogous with that which he sets before us. The alternative, then, which he finally proposes for our choice—namely, that we either allow the modern Hindus to have been able to determine the four occultations by retrospective calculation, or confess the observation a real one, and handed down by tradition through tens of centuries—is one with which we decline to embarrass ourselves: we reject both parts of it, as equally baseless and unnecessary. Prof. Müller is entitled to our gratitude for having drawn out from Mr. Hind the calculations which overthrow Bentley's pretended date, but he needs to excuse himself for attempting to persuade us that they strengthen and establish it.

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* Which Mr. Hind, however, if I rightly understand his figures, makes to have happened an hour or two before sunset in India, so that it could not have been seen there.

In the light of these two exploded hypotheses of Bentley, touching the nomenclature of the months and of the planets, we shall see more clearly in what manner to treat a third similar figment of his, also cited by Müller as "deserving of attention." Bentley interprets the name of the asterism Viçâkhâ as meaning 'divided into two branches or portions,' and declares (putting, as usual, his conjecture in the form of a dogmatic assertion) that it was given to the asterism on account of the latter's being parted exactly through the middle by the equinoctial colure; and as the colure was in position to do so about 1426 B. C., he establishes this as the date of first formation of the system of asterisms. The sufficient answer to this is, that it is the merest etymological conceit, unsupported by a particle of evidence of any kind. It is of no more value than the assertion of the same author that Mûla once led the series of asterisms, because the word *mûla* means 'root,' and that, at another period, Jyeshṭhâ stood at their head, because *jyeshṭhâ* signifies 'oldest.' It is even less plausible: for, in the first place, the most ancient name of the asterism is a dual—*viçâkhe*, 'the two Viçâkhâs'—which proves it to belong, not to the space in the sky defined by the asterism, but to the two stars which compose the group; and, in the second place, the idea that the Hindus, at the period when they selected and named their asterisms, as the first preliminary step (so Bentley describes it) to astronomical observation and reasoning, should have conceived of an equinoctial colure as cutting one of the series in two, and given it an appellation from this circumstance, possesses an incongruity which is almost ludicrous.

I am of opinion, then, that Müller has done a positive disservice to Hindu archæology by his partial rehabilitation of Bentley, in bringing him up to notice anew as an authority worthy of consideration and study, and from whom valuable results may be drawn. If the counter-reasonings given above are sound, we see that all his chosen specimens of Bentley's work are worthless, and that to him, as to others, this author has been a reed which, when leaned upon, has broken and pierced his hand. May we not hope that, if convinced of his error, he will at some suitable opportunity publicly express himself to that effect, and withdraw the partial endorsement he has given, since my voice will reach not a tithe of those who listen with deference to his declared opinions.

I will notice but a single other matter in Prof. Müller's Preface, and one concerning not so much himself, as Colebrooke, from whom he takes it. From Colebrooke's Notes and Illustrations to his Hindu Algebra (Essays, ii. 463), he cites (p. xiv., note) a determination of the age of Brahmagupta upon astronomical evidence. Colebrooke, as is universally known, was a most sound and careful writer, and even his conjectures and crudely formed views upon matters which he was unpossessed of the means of fully judging are always respectable, and, not seldom, intuitive perceptions of the truth as it is later made clearly to appear. But he was not infallible, and our author's quotation gives us one of the most serious of the few errors into which he has fallen. He infers the period of Brahmagupta from an examination of the longitudes of two among the junction-stars of the asterisms, as reported by that astronomer. But Brahmagupta's definition of position of the junction-stars is not different from that which is given by the other astronomical authorities; the slight variations which are to be discovered in the separate accounts are in no degree indicative of independent observation on the part of their authors. So far as there is any evidence before us, we are justified in believing that a single determination of the *dhruvaka* and *vikshepa*, the 'polar longitude' and 'polar latitude,' of these twenty-eight stars, and of half a dozen of the fixed stars not included in the lunar asterisms, made about the year 500 of our era, was the one grand effort, never repeated and never rivalled, of the Hindu astronomy as a science of observation: its results were copied, with occasional slight and unexplained modifications, from author to author, from century to century. The conclusion to be drawn from such calculations as Colebrooke's, then, can only be the date of the observation, and not that of any single authority who reports it: unless Brahmagupta can be shown to be the original observer, or the first reporter—a thing which no one has attempted, and which is very unlikely ever to be done—Colebrooke has no more fixed his period than that, for instance, of the Sûrya-Siddhânta, or of the Graha-Lâghava. This objection, indeed, is urged by Bentley in the concluding Section of his Hindu Astronomy, and constitutes one of the exceedingly few points with regard to which he was in the right, in his repeated and virulent attacks upon Colebrooke: the latter has in vain attempted to turn the force of it in his reply, communicated to the Asiatic Journal in 1826.*

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* Asiatic Journal and Monthly Register for British India and its Dependencies. Vol. xxi., London, 1826. p. 360 etc.